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Abstract: We investigate the impact of various audit schemes on the provision of public goods, when contributing less than the average of the other group members is centrally sanctioned and the probability of an audit is unknown. We study how individuals update their beliefs about the probability of being audited, both before and after audits are definitely withdrawn. We find that when individuals have initially experienced systematic audits, they decrease both their beliefs and their contributions almost immediately after audits are withdrawn. In contrast, when audits were initially less frequent and more irregular, they maintain high beliefs and continue cooperating long after audits have been withdrawn. This identifies the compliance effect of irregularity and uncertainty due to learning difficulties. By increasing both the frequency of audits and the severity of sanctions, we also identify an educative effect of frequent and high sanctions on further cooperation.

Keywords: Ambiguity, audits, sanctions, beliefs, cooperation, public goods, experiment.
JEL Classification: C92, H41, D83

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1. INTRODUCTION

It is well known that the provision of public goods suffers from free-riding (Isaac *et al*. 1986; Andreoni 1988; Ledyard 1995) but that cooperation increases when a decentralized sanction mechanism is introduced (Fehr and Gächter 2000; Masclet *et al*. 2003; Bochet *et al*. 2006; Carpenter 2007). Although this mechanism is inherently uncertain, it improves long-term efficiency (Gächter *et al*. 2008). However, it has also detrimental effects.[1] Replacing peer punishment with legal sanctions (Yamagishi 1986; Polinsky and Shavell 2000; Andreoni and Gee 2012), whether deterrent or not (Tyran and Feld 2006), or with subsidies and taxes (Andreoni and Bergstrom 1995; Falkinger 1996) may avoid these effects. In particular, sanctioning negative deviations and rewarding positive deviations from the mean contribution of group members increase efficiency (Falkinger *et al*. 2000). The mechanism assumes, however, that deviations are measured continuously. Thus, while it removes uncertainty on audits, it may entail excessively high auditing costs and people may suffer from being permanently monitored.

In this paper we study how to improve the efficiency of a centralized sanction mechanism in a public goods game by exploring in a dynamic setting the impact of the (ir-)regularity and the frequency of audits when there is ambiguity about their occurrence. Under ambiguity, can less frequent audits sustain cooperation while minimizing the cost of audits? By ambiguity we mean that individuals are not informed about the probabilities of audits but can infer these from experience (Ellsberg 1961). By varying the pattern of audit sequences, we analyze how

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[1] The positive impact of a decentralized sanction mechanism on cooperation may be higher when sanctions are more severe (Egas and Riedl 2008; Masclet and Villeval 2008; Nikiforakis and Normann 2008). This mechanism may have a detrimental effect because of a deterioration of altruistic cooperation (Fehr and Rockenbach 2003), especially if reprisals are possible (Denant-Boémont *et al*. 2007; Nikiforakis 2008; Nikiforakis and Engelmann 2011), or due to anti-social punishment (Herrmann *et al*. 2008).

individuals adjust their contributions after experiencing a sequence of continuous audits *vs*. a sequence of less frequent and irregular audits. This allows us to measure the residual effects of various policing regimes over time in order to find the optimal interval between successive controls in terms of compliance. Ambiguity, continuity of audits, and alternation of audit regimes capture important real-world features. Indeed, ambiguity prevails, as authorities typically do not provide information about the probability of audits.[2] There are also many examples of systematic audits (*e.g.*, video cameras at traffic lights, screening of passengers at airports, ticket inspection in railways). Crackdowns illustrate continuous/systematic audits in a dynamic framework. They correspond to episodes of very high officer presence, followed by a sudden drop in controls.[3] More generally, auditing policies are often dynamic.[4]

We hypothesize that cooperation can be sustained longer after experiencing irregular audit sequences than continuous audits. If the probability of an audit is unknown, (ir)regularity might condition the capacity to learn the actual probability and thus, impact the evaluation of costs and benefits of contributing. In a different setting it has been shown that under ambiguity, intermittent reinforcers have greater effects on behavior than continuous ones even after a regime

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[2] In the tax domain, the Internal Revenue Service deliberately maintains uncertainty on its audit selection process (Alm *et al*. 1992). The complexity of tax codes and their frequent changes also generate ambiguity. In public transportation, the introduction of inspectors in civil clothes aims at increasing ambiguity. Regarding roadway speeding, mobile radars create ambiguity. Baker *et al*. (2004) give examples of uncertainty in the detection probability in law enforcement. Harel and Segal (1999) show that, if sanctions are usually public, stable and predictable, in contrast authorities manipulate the probability of detection to produce uncertainty for criminals.

[3] Crackdowns are a ubiquitous feature of real world enforcement (Sherman 1990; Di Tella and Schargrodsky 2002, 2004; Kleiman 2009; Eeckhout *et al*. 2010; van der Weele 2012). This applies to tax audits (Slemrod *et al*. 2001), traffic enforcement (speeding, drunk driving, seat-belt wearing, illegal parking), corruption (Di Tella and Schargrodsky, 2002), drug markets (Farrell and Thorne, 2005), and urban violence (Moeller, 2009).

[4] Speed radars or inspections in subways are not always located at the same place, meaning that audits are sometimes systematic and sometimes inexistent. The variations in audit regularity may also be non-deliberate, for example in times of budget restrictions the frequency of tax audits may be reduced. The impact on compliance may differ according to whether audits were previously regular or not.

shift.[5] Similarly, learning is much slower when incentives involve probabilistic rather than continuous reinforcement (Bereby-Meyer and Roth 2006). However, if previous studies have found a positive effect of uncertainty on compliance (Friedland 1982; de Angelo and Charness 2012; Tan and Yim 2013), others have shown that less uncertainty may lead to better outcomes. Alm *et al*. (1992) found that a stronger link between paying taxes and the receipt of public goods leads to a negative effect of uncertainty on compliance. It is thus unclear *a priori* whether ambiguity and higher irregularity of audits does or does not increase contributions to public goods.

To address this question, we have designed a three-player linear public goods game in which negative deviations from the mean contribution of the other group members trigger a centralized sanction in case of an audit. We have implemented a relative mechanism because sanctioning negative deviations is consistent with the centralized sanction mechanism proposed by Falkinger (1996) - except that, for the sake of simplicity, we do not introduce rewards for positive deviations. The advantages of this mechanism are its simplicity and the fact that it does not require more information than other mechanisms (for example, central authorities know the mean reported income in each occupational category; individual deviations from the mean can signal a willingness not to contribute).[6] Also, in sharp contrast with previous literature (Alm *et*

[5] Psychologists have shown that if organisms are continuously reinforced for specific acts during a training phase, then those acts will cease in an extinction phase when the reinforcement is eliminated. However, if reinforcement is irregular during training, the behavior will persist for some time after the reinforcement has been eliminated (Ferster and Skinner 1957; Hilgard and Bower 1975). Hogarth and Villeval (2014) provide an economic analysis of this phenomenon applied to positive incentives.

[6] In the domain of mild laws, many examples also show that sanctions are actually implemented when the proportion of non-compliers exceeds a certain threshold (i.e., the trigger is the relative more than the absolute level of fraud). Tax administrations use risk-based methods to concentrate their audits on high-risk taxpayers who are the most likely to be non-compliant. This scoring includes benchmarking vis-à-vis other companies of similar industries (see Gupta and Nagadevara 2007, Khwaja *et al*. 2011). Evaders may be identified when they report incomes that

al. 1992; Mittone 2006; Kastlunger *et al*. 2009; de Angelo and Charness 2012), players are not informed about the audit probability; they must infer this from experience. Moreover, while the game lasts 50 periods, a regime shift is introduced by eliminating audits after period 22.

Before the regime shift, the frequency of audits and the severity of sanctions vary across treatments. In the *Continuous* treatment, each of the first 22 periods is audited. In the *Intermittent 7* treatment, only seven audits are randomly assigned across periods and the severity of sanctions is tripled (to maintain approximately the same expected cost of sanction for a risk-neutral subject). Notably Egas and Riedl (2008), Masclet and Villeval (2008) and Nikiforakis and Normann (2008) have shown that a sufficiently high sanction coefficient is crucial in sustaining cooperation with peer punishment; it is also important to control for the role of severity in the effectiveness of our centralized sanction mechanism. To disentangle the effect of the irregularity of audits from that of the severity of sanctions, we also manipulate the sanction coefficient. The *Continuous High Fine* treatment is similar to the Continuous treatment except that the severity of sanctions is the same as in Intermittent 7; equivalently, in the *Intermittent 7 Low Fine* treatment the severity is the same as in the Continuous treatment. Finally, we manipulate the frequency of audits by reducing the number of audits to respectively five and three in the *Intermittent 5* and *3* treatments, and to 19 instead of 22 in the *Continuous 19* treatment. This allows us to measure how the irregularity and the frequency of audits affect beliefs and contribution behavior before and after audits are withdrawn definitively, controlling for the severity of sanctions.

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deviate from the mean income of their category. To some extent it is the individual deviation from the average behavior of taxpayers with the same attributes that triggers the audit, and thus the sanction. Here, deviations from the average behavior do not condition the probability of being audited but the intensity of sanctions when audited.

Our results show that when there is ambiguity about the probability of audits, efficiency throughout the game is improved by the use of an intermittent audit scheme, provided frequency of audits and sanctions before the regime shift are sufficiently high. In the Continuous treatment players learn to coordinate on a high contribution level but then free ride immediately after audits are withdrawn. In contrast, although individuals in Intermittent 7 contribute as much as those in the Continuous treatment before the regime shift, the decay of cooperation occurs long after the regime shift and is much smoother. Indeed, players react very little to prediction errors whereas in the Continuous treatment players update beliefs based on the prediction error from the previous period because there is much less variability in the occurrence of audits. Thus, after the regime shift in this treatment beliefs drop sharply immediately and drive contributions downward whereas in Intermittent 7 beliefs do not differ significantly before and after the regime shift and contributions exceeds those in the Continuous treatment. This illustrates the compliance effect of irregular audits and uncertainty.

Increasing the severity of sanctions in the Continuous High Fine treatment does not affect belief formation and increases cooperation before but also, more surprisingly, after the regime shift compared to the Continuous treatment. After the shift, contributions are similar to those in Intermittent 7 using the same sanction coefficient, although the beliefs on audits are lower. This shows that with a high frequency-high severity regime, individuals learn to cooperate because of the higher cost of sanctions and this has some durable effect after beliefs on audits have been revised downward. Taken together, these results reveal both the compliance effect of irregular audits and the educative effect of systematic and severe sanctions. Over the 50 periods, however, efficiency is the highest when audits are intermittent and the severity of sanctions is high. This has important policy implications, especially if one considers the cost of audits in real settings.

In the remainder of our paper, Section 2 surveys the related literature. Section 3 describes the experimental design and procedures and states our predictions. Section 4 reports the experimental results while Section 5 discusses these results and concludes.

2. RELATED LITERATURE

Decentralized sanctions are inherently ambiguous. Indeed, if it is common information that subjects can sanction group members, individuals ignore whether others will be willing to sanction them. However, few have considered the role of ambiguity in social dilemmas. In a prisoner's dilemma game where subjects could decrease the other player's payoff by a random value, Duersch and Servatka (2007) find no effect of uncertainty. In Carpenter (2007), individuals can punish all group members, half of them or only their neighbor. When the number of potential punishers is reduced, it may also reduce uncertainty on being punished. The outcome is a significant increase in free-riding. Public good games with heterogeneous endowments or returns increase uncertainty on punishment, as the norm of contribution in the group is less clear. Decentralized sanctions when norms are more ambiguous are usually less efficient than in a homogenous setting (Reuben and Riedl 2009; Noussair and Tan 2011; Kingsley 2013; Reuben and Riedl 2013), but not systematically (Nikiforakis *et al.* 2010). One study deals directly with a reduction in the ambiguity of sanctions. Masclet, Noussair, and Villeval (2013) allow players to make explicit threats for each possible contribution level of others before the contribution stage. Making threats explicit –reducing ambiguity– increases cooperation. In these papers, however, one cannot identify precisely the role of ambiguity because it is manipulated indirectly through a change in rules; moreover, with decentralized sanction mechanisms, endogeneity issues prevent an analysis of the impact of a change in the demand for punishment on cooperation levels.

In the domain of centralized sanctions, several theories have explored the role of uncertainty on norm compliance. Lazear (2006) shows that to maximize the efficiency of learning, it is better to inform individuals that they will be tested when learning and monitoring are costly, but not when they are easy. Eeckhout *et al*. (2010) propose an incentive-based theory where random crackdowns can be an optimal deterrence policy. A difference with our approach is that crackdowns are publicized while we focus on learning in an ambiguous environment. In law and economics, some theories argue that maximizing uncertainty helps deter crime, as individuals cannot assess precisely the risk of engaging in such actions (Ross 1984; Bebchuk and Kaplow 1992; Harel and Segal 1999; Baker *et al*. 2004). In the tax domain, Snow and Warren (2005a) show that greater uncertainty about audit probability only encourages compliance by ambiguity averse taxpayers. Snow and Warren (2005b) find that more uncertainty about the evaded amount detected by audits can only increase compliance if prudent individuals believe that a substantial fraction is detected. Snow and Warren (2007) show that Bayesian updating of beliefs about the audit probability increases evasion, which suggests that uncertainty could be counterproductive. None of these studies consider regime shifts between different patterns of audits.

Using laboratory experiments, Friedland (1982) has shown that imprecise information on audits increases tax compliance. Comparing a tax regime with a maximum number of audits and strategic uncertainty to a standard flat-rate rule, Tan and Yim (2013) find that uncertainty increases compliance. In these studies, there is no social dilemma. In contrast, comparing the effects of increasing uncertainty between taxation and public goods situations, Alm *et al*. (1992) found that uncertainty increases compliance for the former, but not for the latter. This would be due to the fact that an individual has both to determine his own response to uncertainty and to guess the responses of other group members. In a roadway speeding framing, de Angelo and Charness (2012) implement uncertainty over a deterrence regime by means of compound lotteries and show that increased uncertainty lowers the violation rate (see also Backer *et al*. 2003). On the other hand, Spicer and Thomas (1982) did not find a positive effect of uncertainty on tax compliance. Isaac *et al*. (1989) suggest that audit uncertainty may reduce compliance because individuals perceive the penalties for free riding to be less reliable. Sandler *et al*. (1987) also suggest that tax-rate uncertainty may reduce compliance by making group contributions more uncertain. Our work differs from most of those above in that we introduce a social dilemma and a relative instead of an absolute sanction mechanism. In addition, our aim is not to explore the impact of ambiguity *per se*. Instead, we maintain ambiguity about audits but manipulate their sequence and vary their probability to investigate how compliance is affected by experiencing regular *vs*. irregular audits before audits are withdrawn.[7]

Our approach is closer to that of Kastlunger *et al*. (2009). They compare compliance in a tax game when the audit pattern is continuous at the beginning of the game and intermittent afterwards, and when the audit pattern occurs every second round. Subjects know the mean audit probability but not its distribution over time. Compliance over the life span is higher when audits are concentrated in early as opposed to later periods; it decreases when subjects are not audited over a long period of time. Similarly, by manipulating the sequences of audit we study how past

[7] We also differ from Muehlbacher *et al*. (2012) who show that taxpayers comply more when they have to wait longer before knowing the outcome of an audit. In our paper, ambiguity relates to the probability of an audit, not to the realization of the audit.

experience induces learning. Major differences are that we consider a public goods game and a sanctioning mechanism that punishes relative deviations instead of a fixed tax rate; and our subjects are not informed about the overall probability of audits. Maciejovsky *et al*. (2007) also study the effect of various time lags between audits but without ambiguity.

3. EXPERIMENTAL DESIGN AND PREDICTIONS

3.1. Treatments

Our experiment consists of two main treatments: the *Continuous* treatment and the *Intermittent 7* treatment. In additional treatments, we manipulate the severity of sanctions (*Continuous High Fine* and *Intermittent 7 Low Fine* treatments) and the frequency of audits (*Intermittent 5, Intermittent 3, Continuous 19* treatments).

Continuous treatment. This treatment consists of a linear public goods game played over 50 periods. We form groups of three members that remain fixed throughout the session. At the beginning of each period, each group member is endowed with 20 ECU (*Experimental Currency Units*) and has to decide how many ECU to contribute to a public account. Each period consists of three stages (belief elicitation, contribution, punishment).

In the contribution stage, group members have to choose how many ECU of their endowment (between 0 and 20) to contribute to a group account, with the remainder being kept in their private account. The total amount contributed to the public account by the three group members is shared equally among them and the marginal per capita return of the public account is 0.5. The payoff function for each group member in the contribution stage can be written:

$$p_i^1 = \left(20 - c_i + 0.5 * \sum_{k=1}^{3} c_k \right) \tag{1}$$

where c_i is the individual's contribution, and c_k that of each group member, k= 1,2,3.

The next stage involves sanctions. In some periods, the three group members' contributions are audited exogenously. If the audit reveals that one or two group members have contributed less than the average of the two other group members, a fine reduces their first-stage payoff.[8] The fine is 1.25 times the difference between the mean contribution of the two other group members and the individual's contribution. If there is no audit in the period or if the audit reveals that the individual did not contribute less than the average of the two other group members, the payoff from the contribution stage is not modified. This mechanism differs from that proposed by Falkinger (1996) in that we do not reward positive deviations from the mean contribution.[9] At the end of the punishment stage, the payoff function becomes:

$$\pi_i^2 = \begin{cases} \left(20 - c_i + 0.5 * \sum_{k=1}^{3} c_k\right) - 1.25\,(\bar{c}_{-i} - c_i) & if \quad audit = 1\, and\, c_i < \bar{c}_{-i} \\ \left(20 - c_i + 0.5 * \sum_{k=1}^{3} c_k\right) & if \quad audit = 1\, and\, c_i \geq \bar{c}_{-i}\ or\ audit = 0 \end{cases} \tag{2}$$

where $\bar{c}_{-i}$ is the average contribution of the two other group members.

In addition to strategic uncertainty about others' contributions, the environment is ambiguous. Individuals are not informed about the probability that contributions will be audited in a specific period. Nor do they know *ex ante* the total number of audits in a session. They are only informed at the beginning of the session that the group may be audited in some periods. At

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[8] Two members of the same group can be sanctioned in a period. For example, if a subject contributes 20 and the other members contribute 4 each, then the average contribution of the two others group members is 12 for those who contribute 4. Since the two group members who contribute 4 are below the average, both will be punished if audited.

[9] It should be noted that in the public goods games where sanctions are decided by group members, it is usually observed that group members sanction the individual deviations from the average contribution of others (Fehr and Gächter 2000; Masclet *et al*. 2003). The average contribution is usually considered as the norm of the group, although Carpenter and Matthews (2009) found that absolute norms fit the data better than average contributions.

the end of each period, the participants receive feedback on the total group contribution, the audit realization, and the amount of their own payoff net of the fine, if any. Thus, they have to learn the audit probability from the past sequence of experienced audits. In fact, contributions are audited in each of the first 22 periods. Then, from periods 23 through 50, we remove audits definitively. No participant is informed about this. The objective of the regime shift is to measure the number of periods participants take to realize that there are no more audits.

At the beginning of each period, we elicit the subjects' subjective belief about the occurrence of an audit. Before choosing their contribution, they have to report an integer between 0 and 100 to indicate their belief about the number of chances out of 100 that an audit will occur in the current period.[10] Belief elicitation is incentivized (Gächter and Renner 2010; Wang 2011) according to the quadratic scoring rule as in Nyarko and Schotter (2002). Specifically, participants receive 2 Euros minus a number that varies negatively with the accuracy of their prediction. The penalty is twice the squared deviation between the report and the true outcome. Suppose the reported number is δ, then the payoff is given by:

$$p^P = \begin{cases} 2\text{-}2*(1\text{-}\frac{\partial}{100})^2 \; if \;\; audit = 1 \\ 2\text{-}2*(\frac{\partial}{100})^2 \quad if \;\; audit = 0 \end{cases} \tag{3}$$

Deterministic forecasts of 0 and 100 can lead to the worst and best possible payoffs of €0 and €2. Participants were informed that the best strategy to maximize payoffs is to state their true belief about the number of chances that an audit will occur.

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[10] Eliciting beliefs about the probability of being audited instead of simply asking whether subjects believe that this period will be audited or not causes less identification problems (see Manski 2004). However, with quadratic scoring rules, risk aversion increases the likelihood that subjects report a probability of 0.5.

Participants are paid the sum of the payoffs for each of the 50 periods of the game plus the payoff from the prediction made in one of the 50 periods, randomly drawn at the end of the session.[11] We only pay for one prediction to limit the risk of hedging (see Blanco *et al*. 2010).

Intermittent 7 treatment. This treatment introduces two changes compared with the Continuous treatment. The first is that audits occur intermittently before the regime shift. On average, there is an audit every three periods and we impose that seven periods are audited in each group in the first 22 periods. This includes an audit in period 22 such that the last audited period is the same across treatments. The other audits are independently and randomly distributed in the groups.[12] As in the Continuous treatment, no more audits occur after period 22, and the number, frequency and distribution of audits are not made common knowledge. The second change is that the sanction coefficient is three times higher than in the Continuous treatment (3.75 instead of 1.25). We compensate the lower probability of an audit with a higher sanction, so that the expected cost of a sanction is similar across treatments for a risk-neutral individual. The payoff function is:

$$p_i^2 = \begin{cases} \left(20 - c_i + 0.5 * \sum_{k=1}^{3} c_k\right) - 3.75\,(\bar{c}_{-i} - c_i) & if \quad audit = 1\, and\, c_i < \bar{c}_{-i} \\ \left(20 - c_i + 0.5 * \sum_{k=1}^{3} c_k\right) & if \quad audit = 1\, and\, c_i \geq \bar{c}_{-i}\; or\; audit = 0 \end{cases} \tag{4}$$

The strategic uncertainty

[11] We paid the sum of the payoffs in the 50 periods instead of paying one randomly drawn period to avoid adding uncertainty in payoffs to the uncertainty manipulated in the experimental protocol. We acknowledge that this choice also has some drawbacks: it may create wealth effects, the stake of each period is low and it may not be incentive compatible if a decision in one period is distorted by decisions in the other periods (Azrieli *et al*. 2012). However, our main aim is comparing treatments, so this choice should not affect our main results.

[12] In the Intermittent 7 treatment the expected level of sanctions is 26.25 (3.75*7=26.25) times the deviations. By implementing audits in 22 periods in the Continuous treatment, the expected level of sanctions is 27.5 (1.25*22 = 27.5) times the deviations. We acknowledge that we could have avoided this difference by imposing audits in 21 periods instead of 22. The reason for the choice of 22 periods is that initially, we planned to run an intermittent treatment with regular audit patterns (having an audit every 3 periods starting in period 1 required 22 periods). This difference should reduce the relative effectiveness of the Intermittent 7 treatment compared to the Continuous one. Our results may thus possibly underestimate the relative advantage of the Intermittent 7 treatment.

is the same as in the Continuous treatment. So if we observe behavioral differences between this treatment and the previous one, they should be attributed to the individuals' reactions to the intermittence of audits.

Additional treatments varying the severity of sanctions. The two previous treatments hold constant the expected cost of sanction in each period across treatments, but they differ on two dimensions: the probability of apprehension and the severity of the sanction coefficient. To disentangle the respective effects of the probability of audits and of the sanction coefficient we have designed a *Continuous High Fine* treatment. This treatment is similar to the Continuous treatment except that the sanction coefficient is 3.75 as in Intermittent 7. Equivalently, the *Intermittent 7 Low Fine* treatment is similar to Intermittent 7 except that the sanction coefficient is 1.25 as in the Continuous treatment.

Additional treatments varying the frequency of audits. To study the sensitivity of contributions to the frequency of audits and to help determine how frequent audits have to be to produce high compliance, we designed *three* other treatments with a lower number of audits. The *Intermittent 5* and *Intermittent 3* treatments are similar to Intermittent 7, except that only five and three audits, respectively, occur in the first 22 periods instead of seven. As before, this is not made common knowledge and the sanction coefficient is 3.75. The *Continuous 19* treatment is similar to the Continuous treatment with the 1.25 sanction coefficient, except that the probability of an audit is no longer 100% before the regime shift, but 85% (*i.e.* we conduct 19 audits instead of 22 between periods 1 and 22, so that it remains 'almost continuous').

Elicitation of attitudes towards, risk, ambiguity and losses. At the beginning of the sessions, we elicited participants' attitudes toward risk and uncertainty by asking them to price both a clear bet and a vague bet following a procedure similar to that of Fox and Tversky (1995).[13,14] One of the two sets of decisions was randomly drawn for payment at the end of the session. Then, loss aversion in risky choice tasks was elicited using the same procedure as in Gächter *et al*. (2010).[15] These decisions were made at the beginning of the session but their outcomes were determined only after the public goods game had been completed (thus avoiding income effects).

3.2. Predictions

Let us first assume a single-shot game and that players are able to anticipate whether an audit occurs or not. Sanctions are credible because they are exogenously imposed. In the case of no audit, free-riding is a dominant strategy since the marginal per capital return from the group account is smaller than that from the private account. In case of an audit, each possible level of contribution is a Nash equilibrium, as there is no sanction if all contribute the same. Full contribution is a payoff dominant Nash equilibrium. Let us now assume that individuals have the same prior beliefs about the audit probability and they are risk neutral. In the Continuous,

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[13] Subjects make a first set of 20 decisions between accepting a certain payoff and drawing a ball in an urn that contains 5 blue balls and 5 yellow balls (a risky lottery). The amount of the certain payoff increases from 0.25 eurocents to €5. One yellow ball drawn from the run pays €5, a blue ball pays nothing. Then, subjects have to make a second set of 20 similar decisions except that the proportions of yellow and blue balls in the urn are now unknown (an ambiguous lottery). At the end of the session, the program randomly determines which urn is used for payment. For this urn it randomly draws a number between 1 and 20 to determine which of the 20 decisions matters for determining the participant's earnings. If the participant has chosen the certain amount, this amount is added to his other earnings. If he has instead chosen to draw a ball, the program draws a ball from the selected urn. If a yellow ball is drawn, it pays €5. In both sets of decisions, a risk neutral participant should choose to draw a ball from the urn until the certain payoff is equal to at least €2.5. In the first set of decisions, a risk averse (seeking) subject should switch to the certain payoff for lower (higher) certain amounts. An ambiguity averse (seeking) subject should switch for lower (higher) certain amounts in the second set of decisions than in the first one.

[14] We acknowledge that it could have been more relevant to elicit attitudes towards ambiguity in the domain of losses instead of in the domain of gains. However, our procedure allowed us to use the same lotteries to elicit the attitudes towards risk and ambiguity. This kept the procedure simple.

[15] Each participant makes six successive decisions between participating or not in a lottery. In the lottery choices the winner's prize is fixed at €6 whereas the loser's prize increases incrementally from -€2 in the first lottery choice to -€7 in the sixth lottery choice. Refusing to participate in a lottery guarantees the individual that he will earn nothing and lose nothing. Loss neutrality supports the decision to play the first five lotteries since their expected payoff is positive or null. Loss aversion can lead the participant to reject other lotteries.

Continuous 19 and in Intermittent 7 Low Fine treatments, they should contribute 20 if they believe that the audit probability is greater than 40%, and 0 otherwise. In the Continuous High Fine and Intermittent 7/5/3 treatments, they should contribute 20 if they believe that the audit probability is greater than 13%, and 0 otherwise (see Appendix A for the proof). Using backward induction, the single shot outcome is expected also in the repeated game because this game is finitely repeated. Note that since groups are fixed throughout the game, behavior may also be driven by reputation building. This effect should, however, be similar across treatments.

Participants have to infer the actual audit probability from their past experience. The investigation of how beliefs are updated is crucial to predict behavior. We assume that at the beginning of each period, participants update their belief based on their past experience of audits up to that point. We further assume that they are boundedly rational in that they update their belief not according to Bayes' rule[16] but to a less cognitively demanding Bayesian-like process (see Hogarth and Einhorn 1992, and Hogarth and Villeval 2014). In this *anchoring and adjustment* process, judgment is anchored on the previous belief and updated by the experience of the latest period. This is expressed as follows (see Hogarth and Villeval 2014):

$$S_k = S_{k-1} + w_k\,[s(x_k) - S_{k-1}] \tag{5}$$

where S_k is the belief that the next period will be audited after experiencing k periods with $0 \leq S_k \leq 1$; S_{k-1} is the belief that the previous period would be audited; $s(x_k)$ indicates whether the k^{th} period was audited (with $s(x_k) = 1$) or not (with $s(x_k) = 0$); w_k is an adjustment parameter that

[16] Gilboa *et al.* (2008) argue that the Bayesian model is too restrictive in that beliefs are seldom specific enough to define a unique probability distribution and too general in that it offers no insight into how prior beliefs are formed.

determines how the latest evidence modifies the previous assessment.[17] It is assumed proportional to S_{k-1} when $[s(x_k) - S_{k-1}] \leq 0$. Thus,

$$w_k = \alpha S_{k-1} \quad \text{when } [s(x_k) - S_{k-1}] \quad \leq 0 \tag{6}$$

and

$$w_k = \beta(1\text{-}S_{k-1}) \quad \text{when } [s(x_k) - S_{k-1}] \quad > 0, \quad \text{where } 0 \leq \alpha, \beta \leq 1. \tag{7}$$

The model can be rewritten as:

$$S_k = S_{k-1} + \alpha\, S_{k-1}\, [s(x_k) - S_{k-1}] \qquad \text{when } [s(x_k) - S_{k-1}] \quad \leq 0 \tag{8}$$

and

$$S_k = S_{k-1} + \beta\, (1\text{-}S_{k-1})\, [s(x_k) - S_{k-1}] \qquad \text{when } [s(x_k) - S_{k-1}] \quad > 0 \tag{9}$$

α and β represent the players' attitudes toward over- or under- predictions of audits, respectively, and we expect that α and β should be higher when the experienced variability is low, as in the Continuous treatments. That is, here players put more weight on the most recent evidence than in the Intermittent treatments because less information is needed to update beliefs. In contrast, in the Intermittent treatments players should place more weight on the anchor S_{k-1} than on recent evidence because more information is needed to make an assessment. Figure 1 displays the outcomes of the simulations of these assumptions over 50 periods, with α=β=0.5 in the Continuous treatment and =0.2 in the Intermittent 7 treatment.[18] For the latter, we have averaged the outcomes of 30 random simulations. Figure 1 shows that our model predicts different patterns. In the Continuous treatment, the beliefs about the occurrence of an audit increase in the early periods. Then, they drop sharply immediately after audits stop. In the Intermittent 7 treatment, there is a constant pattern through period 22 after which the evolution

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[17] This is similar to reward prediction error models of learning (Schultz *et al*. 1997; Montague *et al*. 1996; Caplin and Dean 2007) and structurally similar to the EWA model of reinforcement (March 1996; Camerer and Ho 1999).

[18] We have also made simulations with α>β and α<β and with other values of α and β to test the sensitivity of the model's predictions (available upon request). The predicted patterns of beliefs are quite robust to these variations.

of beliefs is smoother and beliefs stay higher than in the Continuous treatment.

[Insert Figure 1 about here]

Based on this simple model, we derive the following hypotheses.[19]

Hypothesis 1. The pattern of belief updating differs across treatments. The occurrence of an unexpected event in the previous period has a stronger influence on the evolution of beliefs in the Continuous treatments than in the Intermittent treatments, whereas the weight of the anchor matters more than the surprise effect in the previous period in the Intermittent treatments.

Hypothesis 2. In the Continuous treatments, beliefs on audits increase in the early periods and drop immediately after period 22 when audits actually stop.

Hypothesis 3. In the Intermittent 7 treatment, there is a constant pattern through period 22 after which there is a slowly decreasing trend toward period 50. Beliefs are higher than those in the Continuous treatment after the regime shift. The evolution of beliefs in the other Intermittent treatments follow the same pattern as in the Intermittent 7 treatment; the only difference is the mean level of the beliefs.

If we assume that individuals are risk neutral and have the same prior beliefs and if we use payoff dominance, the following hypotheses can be made concerning contribution behavior.

Hypothesis 4. In the Continuous treatment (Continuous High Fine treatment, respectively), individuals contribute their whole endowment when they believe that the probability of being audited exceeds 40% (13%, resp.), and nothing otherwise. Before the regime shift, their contributions become increasingly full; after the regime shift, they free-ride. The same evolution pattern is predicted in the Continuous 19 treatment.

Hypothesis 5. In the Intermittent treatment 7, individuals contribute their whole endowment if they believe that the probability of being audited exceeds 13%, and nothing otherwise. Compared to the Continuous treatment, cooperation is observed longer after the regime shift. Contributions in the Intermittent 5 and 3 treatments follow the same pattern. Free-riding develops much earlier in the Intermittent 7 Low Fine.

Hypothesis 6. Risk, ambiguity, and loss aversion increase predicted contributions in all treatments.

3.3. Procedures

The participants were 291 undergraduate students from local engineering and business schools

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[19] We could consider alternatively a model in which individuals are able to recognize temporal patterns in the audit realization, instead of assuming that individuals update their beliefs sequentially. In a companion paper (Hogarth and Villeval 2014), we consider such an alternative model and show that it delivers qualitatively similar predictions although its complexity is dramatically higher. Note that this game also has a number of mixed equilibria.

who were invited via the ORSEE software (Greiner 2004). 18 sessions were conducted at the GATE (*Groupe d'Analyse et de Théorie Economique*) research institute in Lyon, France. We ran four sessions of each of the Continuous and Intermittent 7 treatments, and two sessions of each of the Continuous High Fine, Continuous 19, Intermittent 5/3/7 Low Fine treatments. To check whether belief elicitation might affect contributions, beliefs about the occurrence of audits were elicited in half of the sessions with the Continuous and Intermittent 7 treatments.[20] In all other sessions, beliefs were systematically elicited. Table 1 displays summary information.

[Insert Table 1 about here]

Upon arrival, participants were randomly assigned to a terminal by drawing a tag from a bag. The instructions for each part were distributed and read aloud after completion of the previous part (see Appendix B). Questions were answered in private. Subjects' understanding was checked before the experiment started. At the end of the session, participants had to complete a demographic questionnaire. It was made common information that a secretary who was not aware of the content of the experiment paid subjects in cash and in private in a separate room.

Sessions lasted approximately 60 minutes excluding payment. The average payment was €16.89 (standard deviation €4.40), including a €4 show-up fee and the earnings from either the risky or the ambiguous lottery task and from the loss aversion task.

4. RESULTS

We first analyze the evolution of beliefs over time before examining how contributions depend

[20] Gächter and Renner (2010) suggest that eliciting incentivized beliefs increases contribution levels relative to a benchmark treatment without belief elicitation, while a contrary result is reported by Croson (2000). A difference is that in our design, we do not elicit beliefs about others' contributions but on the probability of being audited.

on the audit regime. Last, we compare earnings in the various treatments.

4.1. Beliefs

Table 2 summarizes the mean beliefs by treatment, before and after audits are withdrawn. The two panels of Figure 2 display the relative frequencies of each belief in our main treatments (Continuous and Intermittent 7), before and after the regime shift. Finally, the three panels of Figure 3 display the evolution of beliefs over time in the various treatments.

[Insert Table 2 and Figures 2 and 3 about here]

Not surprisingly, Table 2 indicates that mean beliefs are higher in the Continuous than in the Intermittent 7 treatment in the first 22 periods. Interestingly, the opposite is true in periods 23-50 although both treatments are now identical with respect to audits. Mann-Whitney tests (MW, hereafter) show that these differences are significant both in periods 1-22 (p=0.001) and in periods 23-50 (p=0.029).[21] Reducing the frequency of audits in the Continuous 19 treatment has a negative impact on beliefs before the regime shift (p=0.035) and a positive impact after the regime shift (p=0.068), indicating that subjects learn slightly less rapidly than in the baseline. In the other Intermittent treatments, beliefs differ significantly from those in the Continuous treatment in the first set of periods (p=0.001 in both Intermittent 5 and 3; p=0.003 in Intermittent 7 Low Fine), but not in the second set (p>0.100). Increasing the severity of sanctions in the Continuous High Fine treatment has no significant impact on beliefs (p>0.10).

The distributions of beliefs differ across treatments.[22] In the Continuous treatment a

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[21] In all the non-parametric tests reported in this paper, the belief of a group of three members averaged across a block of periods (either periods 1-22 or periods 23-50) constitutes one unit of observation. Tests are two-tailed.

[22] Kolmogorov-Smirnov tests (KS, hereafter) indicate that the distributions of beliefs in the Continuous and Intermittent 7 treatments differ significantly (p=0.002 for periods 1-22, p=0.029 for periods 23-50). The comparison between the Continuous and the other treatments shows differences before the regime shift (p=0.006 in Intermittent 5,

majority of individuals underestimate the audit probability in the first set of periods (64%) and, compared to the Intermittent 7 treatment, a lower proportion of individuals overestimate the probability of an audit after period 22 (59%). Indeed, in the Intermittent 7 treatment, a large majority of individuals overestimate the probability of an audit both before (71%) and after the regime shift (84%). In fact, Figure 2 shows three peaks in the distribution of beliefs in both treatments, at 0, 50 and 100. In periods 1-22, these values represent respectively 9%, 26% and 36% of the observations in the Continuous treatment, and 10%, 35% and 10% of the observations in the Intermittent 7 treatment. In periods 23-50, the corresponding percentages are 41%, 20% and 9% in the Continuous treatment and 16%, 32% and 6% in the Intermittent 7 treatment, despite the absence of an audit. The relatively high frequency of reports at 50 may be driven by risk aversion, since the use of the quadratic scoring rule makes it risky to report extreme values.

Figure 3a shows that the evolution of beliefs over time is consistent with the simulation of our model (Figure 1). Indeed, in the Continuous treatment beliefs increase progressively in the early periods and they decrease sharply immediately after the regime shift, without matching the actual probabilities. Then, they stabilize at a positive level through the last period, as some players still expect audits to happen. Wilcoxon tests show that beliefs differ before and after the regime shift (p=0.004). In Intermittent 7 the pattern is different. Beliefs are relatively stable throughout the game with no visible shift when audits are withdrawn (p=0.475).[23] Beliefs

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0.001 in Intermittent 3, and 0.004 in Intermittent 7 Low Fine), but not after (p=0.461, 0.979, and 0.195, respectively).

[23] Figure 3a shows a drop in beliefs in period 23 in Intermittent 7. This can be explained by the fact that all groups were audited in period 22 and by the so-called "bomb crater" effect (Guala and Mittone 2005; Mittone 2006).

decrease smoothly after period 30 but the mean belief in the last period still corresponds to the actual audit probability before the regime shift. Manipulating the severity of sanctions has no impact on the evolution of beliefs over time (see Figure 3b): beliefs are significantly lower after than before the regime shift in the Continuous High Fine like in the Continuous treatment (p=0.003); they do not differ significantly over time in the Intermittent Low Fine (p=0.110), like in Intermittent 7. In the other Intermittent treatments in which we reduce the frequency of audits, we observe the same apparently stable pattern as in Intermittent 7 (see Figure 3c) but Wilcoxon tests show that beliefs differ before and after the regime shift (p=0.033 in Intermittent 5 and 0.051 in Intermittent 3). Finally, in the Continuous 19 treatment, beliefs are significantly lower after than before the regime shift (p=0.003).

To test our model formally, Table 3 reports estimates of the determinants of beliefs from various Tobit models (since data are censored both on the left and the right).[24] Robust standard errors are clustered at the group level. The first three models pool all treatments together and dummy variables for each treatment are included as independent variables, with the Continuous treatment taken as the reference. Model (1) considers periods 1-22 and model (2) periods 23-50; model (3) pools all the periods. Models (4) to (10) test our model for each treatment separately. The independent variables include the belief in t-1 (the anchor variable) and two variables that reflect reactions to prediction error in the previous period. The negative prediction error variable is defined as $S_{k-1}\ [s(x_k) - S_{k-1}]$ when no audit occurred in the previous period, and 0 otherwise. The positive prediction error variable is defined as $(1-S_{k-1})\ [s(x_k) - S_{k-1}]$ when an audit occurred,

[24] OLS estimates offer similar qualitative results.

and 0 otherwise. In all models we include a loss index variable, defined by the switching point in the loss aversion lottery. The risk index variable takes the value of the switching point in the risky lottery. The higher these values, the less loss or risk averse is the individual. The ambiguity aversion variable represents the difference in switching points in the risky and the ambiguous lotteries. A positive value means that the individual has switched from the lottery to the certain equivalent earlier in the ambiguous than in the risky decision task, thereby indicating ambiguity aversion. Finally, we control for gender and age, and include a time trend.[25]

[Insert Table 3 about here]

In the first set of periods (model (1)), Table 3 confirms that, compared with the Continuous treatment, beliefs are significantly lower in all the intermittent treatments and in the Continuous 19 treatment. The severity of sanctions does not affect beliefs: the coefficient of Continuous High Fine is not significant and the coefficient of the Intermittent 7 variable does not differ from that of Intermittent 7 Low Fine treatment (p=0.779). The coefficients of Intermittent 7, Intermittent 5 and Intermittent 3 do not differ either (p=0.989 and p=0.246, respectively).

In contrast, in the second set of periods (model (2)), an important finding is that beliefs are now significantly higher in Intermittent 7 than in the Continuous treatment. The coefficient of Intermittent 7 differs also significantly from those of Intermittent 3 and Continuous High Fine (p<0.001), but not from the coefficients of Intermittent 5 and Intermittent 7 Low Fine (p=0.450 and p=0.511, respectively), confirming that beliefs are not affected by the severity of sanctions.

Models (4) to (8) support our theoretical prediction that when variability is low, more weight

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[25] In other regressions, we included dummy variables for being punished in the previous period. Since the effect of punishment on beliefs was not significant, we do not report these regressions here.

is put on the most recent evidence to form one's beliefs compared with the treatments with higher variability that require information from more past periods. Indeed, in the Continuous treatment, both negative (in periods 1-22) and positive (in periods 23-50) prediction errors in the most recent period affect beliefs significantly; both types of error have the same impact ($\alpha=\beta$, p=0.152). In contrast, in most intermittent treatments only the anchor is significantly different from 0 while prediction errors in t-1 do not affect the current beliefs. A negative prediction error has a marginally significant impact only in Intermittent 5 (model (8)): overestimating the probability of an audit in t-1 reduces the belief at t.[26] Perhaps surprisingly, we do not find a higher anchor coefficient in the Intermittent than in the Continuous treatments; this probably suggests that beliefs are more volatile in this more unstable environment.

Finally, a higher degree of risk aversion (denoted by a negative coefficient of the risk index) and ambiguity aversion (denoted by a positive coefficient of the ambiguity variable) increases the subjective belief on the occurrence of an audit in Intermittent 7. We also find that in the Continuous 19 treatment more risk averse and less loss averse players hold higher beliefs.[27]

We summarize our results on beliefs as follows.

Result 1. Prediction errors in the previous period influence current beliefs in the Continuous treatment but not in the Intermittent treatments where beliefs need to account for more periods to summarize the past history of the game. This supports Hypothesis 1.

Result 2. In the Continuous treatment, assessments of the probability of an audit drop sharply immediately after the regime shift. This supports Hypothesis 2. The process is slightly slower in the Continuous 19 treatment.

Result 3. In contrast, beliefs in the Intermittent 7 treatment decrease slowly and smoothly after the regime shift; after audits are withdrawn, beliefs are not significantly different from beliefs

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[26] In the regression the sign is positive but the variable is by construction negative.

[27] We also tested models (1-3) with interaction terms between the loss/risk/ambiguity aversion variables and a dummy variable for the intermittent treatments. Almost none of these interaction terms were significant.

before the regime shift and they exceed those in the Continuous treatment through the end of the game. This supports Hypothesis 3.

Result 4. Reducing the frequency of audits from 7 to 5 or varying the severity of sanctions have limited or no impact on belief formation.

4.2. Contributions

Table 2 summarizes the mean contributions by treatment both before and after audits are withdrawn. It reveals that in periods 1-22 the mean contributions in the Continuous (15.3 ECU) and in the Intermittent 7 treatments (15.4) do not differ (MW, p=0.573); the distributions are also similar (KS, p=0.319).[28] Thus, although learning is imperfect, reducing the audit probability by two thirds and tripling the amount of the fine leads to similar high levels of cooperation as if there was no uncertainty. In contrast and importantly, while after period 22 the mean contribution decreases to 8.2 in the Continuous treatment, it remains high at 13.9 in Intermittent 7. The differences in the means and distributions are highly significant (MW and KS, p<0.001).

For a given frequency of audits, the severity of sanctions matters. The mean contribution in Continuous High Fine is higher than in the Continuous treatment both before the regime shift (18.1, MW: p=0.006) and after (12.8, p=0.055). This result is expected since for similar beliefs, the expected cost of sanctions is higher. For the same reason, contributions are lower in Intermittent Low Fine than in Intermittent 7 (11.7, p=0.034 before the shift; 8.4, p=0.007 after).

For a given severity of sanctions, the frequency of audits matters as well. Mean contributions are unchanged when the number of audits decreases from 22 to 19 (12.8, p=0.311 before the shift; 10.3, p=0.664 after) or from seven to five (14.9, p=0.673 before; 12.5, p=0.398

[28] No significant difference on average contribution was found between the Continuous treatments with and without belief elicitation. Therefore, we pool together the data from the Continuous treatments in this analysis. For the same reason we pool together all the data from the Intermittent 7 treatments with and without belief elicitation.

after). However, there is a limit. Reducing this number from seven to three cannot sustain the same level of contribution (12.2, p=0.050 before; 9.8, p=0.060 after). More importantly, comparing the Continuous High Fine with the Intermittent 7 treatments indicates that contributions are higher when audits are systematic before the shift (p=0.019), due to more frequent sanctions. After the shift, contributions are higher in Intermittent 7 but not significantly so (p=0.482). This relatively high level of cooperation in Continuous High Fine after the shift is somewhat surprising since the sanction coefficient and the frequency of audits are similar in both treatments but the beliefs about the occurrence of an audit are lower in this treatment than in Intermittent 7. This shows that the history of cooperation in groups matters beyond a change in the environment. With a high severity-high frequency regime, individuals learn to comply at a high level because of the continuous auditing scheme and this has some durable effect after they have revised their beliefs downward about the occurrence of an audit. This is the *educative effect* of high frequency-high severity policing. With more irregularity, people cooperate durably for a different reason: they fail in updating their beliefs. This is the *compliance effect* of irregularity.

The three panels of Figure 4 display the evolution of contributions over time.

[Insert Figure 4 about here]

Figure 4a shows that contributions in the Continuous treatment increase regularly before the regime shift; afterwards, they decrease rapidly as beliefs of an audit drop, and free-riding develops as in a standard public goods game. In period 50, the mean contribution is only 3.3 (S.D.=5.6). In contrast, in Intermittent 7 contributions are stable from the beginning and only start to decrease after period 36. In period 50, the mean contribution is still 10.3 (S.D.=7.9), which is significantly higher than in the Continuous treatment (p<0.001). Figure 4b suggests that a higher severity of sanctions in the Continuous High Fine treatment cannot prevent a decline of contributions over time but the decline seems more moderate than in the Continuous treatment after the regime shift. Finally, Figure 4c shows that the patterns of contributions look similar in the Intermittent 7 and 5 treatments and that even in Intermittent 3 the evolution is quite slow.

To study the role of beliefs, pattern of audits, and severity of sanctions on the contribution decision, Table 4 reports the estimates of Tobit models with robust standard errors clustered at the group level. Models (1) and (2) concern periods 1-22, models (3) and (4) periods 23-50, and models (5) and (6) the whole session. The independent variables include treatment dummies (with the Continuous treatment as the reference), the other group members' mean contribution in t-1 (indicating the degree of group cooperation), the individual's attitudes towards loss, risk and uncertainty, gender, age, and finally, a time trend. In addition, we include either a dummy variable to control for a possible effect of belief elicitation on contributions in models (1), (3) and (5) or the belief itself in models (2), (4) and (6).

[Insert Table 4 about here]

Table 4 confirms first that beyond beliefs, contributions depend strongly on the intermittence of the audit regime. In periods 1-22, there is no significant difference in the contribution level between Intermittent 7 and the Continuous treatment. In contrast, in periods 23-50 the contribution is higher in the Intermittent 7 treatment than in the Continuous treatment. Second, while the severity of sanctions did not affect beliefs, a higher sanction coefficient increases contributions because free-riders are price-sensitive: the coefficient of Continuous High Fine is significant both before and after the regime shift, while the coefficient of the Intermittent 7 Low Fine variable differs from that of Intermittent 7 (p=0.006 before the regime shift and p=0.052

after the shift). Third, having 19 or 22 audits does not make any difference in contributions in the Continuous treatments; there is no difference either between the coefficients of the Intermittent 5 and Intermittent 7 variables (except that after the regime shift when we control for beliefs, the coefficient of Intermittent 5 does not differ significantly from that of the Continuous treatment variable). A frequency below 5 reduces contributions only before the regime shift. Fourth, the more other group members cooperate, the more the individual also contributes. Besides the fact that cooperation is conditional, the norm of matching the mean contribution of others is stated explicitly and the deviation from this mean determines the level of the fine. Finally, models (3) and (4) show that ambiguity aversion increases contributions.[29]

To assess how contributions adjust to changes in beliefs, Table 5 reports the estimates of Tobit models in which the dependent variable is the evolution of the contribution between *t*-1 and *t*. Model (1) concerns periods 1-22, model (2) periods 23-50, and model (3) pools all the periods together. The independent variables include the variation of beliefs between *t*-1 and *t* and a sanction variable in *t*-1, both being also interacted with each treatment. We control for the mean contribution of the others in *t*-1, for individual characteristics, and for time.

[Insert Table 5 about here]

Table 5 shows that contributions increase between two periods in reaction to an increase in the belief of being audited and to a sanction in *t*-1 (model (1)). There is a large additional impact of the variation of beliefs in Intermittent 7 but only in periods 1-22. This additional impact is probably not due to the perspective of a higher fine in case of a lower contribution than the

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[29] Adding in these models interaction terms between risk/ambiguity/loss indices and one dummy variable for all the intermittent treatments indicates that in these treatments risk averse subjects contribute more throughout the game, while ambiguity averse subjects contribute more only after the regime shift.

average of the two other group members since the coefficient is also significant in Intermittent Low Fine and not in Continuous High Fine. After the regime shift (model (2)) and if we consider the 50 periods together (model (3)), the evolution of beliefs has the same effect on contributions across treatments. Since beliefs react less to immediate prediction errors in the intermittent treatments, contributions decrease less in the Intermittent than in the Continuous treatments.

We summarize our main results on contributions as follows.

Result 5. In the Continuous treatment, players progressively coordinate on a high contribution level before the regime shift. After the regime shift they rapidly engage in free-riding, consistently with a rapid change in beliefs. This supports Hypothesis 4.

Result 6. In Intermittent 7, individuals contribute as much as in the Continuous treatment before the regime shift. After the shift, cooperation becomes higher, notably due to low reactions to prediction errors. This supports Hypothesis 5 and captures the *compliance effect* of irregularity and uncertainty.

Result 7. A combination of high frequency of audits – high severity of sanctions has some *educative effect* on cooperation. Reducing the frequency of audits from 22 to 19 in the Continuous treatments or from 7 to 5 in the intermittent treatments does not affect contributions. Reducing the frequency of audits further in the intermittent environment or reducing the severity of sanctions lowers contributions. This also supports Hypotheses 4 and 5.

Result 8. Risk and loss aversion have limited impact on contributions. Supporting only partly Hypothesis 6, ambiguity averse individuals contribute more than others after the regime shift.

4.3. Efficiency

Table 2 summarizes mean payoffs (net earnings) per period before the regime shift, after the shift and for all periods, by treatment. It shows that differences in payoffs between treatments emerge after audits have been withdrawn. In the Intermittent 7 and 5 and Continuous High Fine treatments, earnings increase after the regime shift as group members still cooperate but no longer pay fines. In contrast, in the Continuous treatment payoffs decrease because free-riding develops rapidly. In periods 23-50 mean payoffs are higher in Intermittent 7 and 5 than in the

Continuous treatment (MW, $p<0.001$ and 0.014, respectively); payoffs in Intermittent 7 are higher than in the Continuous High Fine treatment, but not significantly so. When the 50 periods are considered, payoffs are significantly higher only in the Intermittent 7 treatment ($p=0.030$).[30] Reducing the amount of the fine cannot maintain the beneficial impact of intermittent audits.

This supports our last result.

Result 9: Under ambiguity, the use of an intermittent audit scheme increases efficiency over the 50 periods of the game.[31] However, the frequency of audits and the level of fines must be high enough to produce this efficiency gain.

5. DISCUSSION AND CONCLUSION

We provide the first systematic evidence comparing the effects of continuous and intermittent audit schemes on the enforcement of cooperation in the provision of public goods under ambiguity on the audit regime. Under ambiguity for a similar expected cost of sanctions (i.e. high fine with low probability *vs*. low fine with high probability), an intermittent audit policy is more effective in sustaining contributions than a succession of systematic then rare audit policies because individuals are less able to update their beliefs in an irregular environment. This suggests a compliance effect of uncertainty and irregularity due to learning difficulties.

However, when increasing the expected cost of sanctions, we found that a policy based on systematic audits and severe sanctions also has a positive effect on contributions even after the withdrawal of audits. Although individuals are more able to update their beliefs on audits than in

[30] We ignore what would be the mean payoffs in the absence of sanction. We can, however, compare our data to those of an experiment with similar parameters but where no punishment is enforced like Gächter *et al*. (2008) where the mean payoff is 23.60. Using two-tailed *t*-tests, we find that mean payoffs are significantly higher both in our Continuous (N=21; payoff=24.87; $p=0.003$) and Intermittent 7 treatments (N=20; payoff=26.11; $p<0.001$). This could possibly suggest that our schemes leave subjects better off than in an environment with no sanction.

[31] We acknowledge that using earnings as a measure of efficiency disregards the welfare losses for individuals who dislike uncertainty and suffer from this situation.

an irregular environment, they have been used to comply at a higher level and they keep contributing more than individuals who were not exposed to severe and frequent sanctions before. This finding suggests that a high frequency-high severity policy has an educative effect.

Studies like Egas and Riedl (2008), Masclet and Villeval (2008), and Nikiforakis and Normann (2008) have shown that the severity of the sanction parameter is crucial in sustaining cooperation in public good games with decentralized sanctions. Our results show that more severe centralized sanctions are also more effective but also that for a given severity, withdrawing sanctions for a time may have limited influence on contributions when the environment makes it more difficult for individuals to learn the true probability of audits.

Although we must remain cautious before extrapolating our findings to real settings, this research may have interesting implications in terms of public policies in times of scarce resources and budget deficits. If a policy based on systematic audits and severe sanctions supports high compliance levels, it comes at a cost since audits are expensive. If we had to account for auditing costs in our experiment, then our conclusion would reinforce the recommendation in favor of more irregular and less frequent audits with severe sanctions since the threat of audits is persistent.[32] Restrictions in auditing budgets do not necessarily lead to an explosion of fraud since individuals do not realize immediately that a regime shift has occurred.

An interesting extension would be to test the robustness of our conclusions to an absolute sanction mechanism. Indeed, our findings contrast with Alm *et al*. (1992) who found that introducing uncertainty in a tax experiment with public goods reduced compliance, which could

[32] Of course, other considerations might play a role in the choice of systematic audits, like security in the case of screening at airports, or service to the clients in the case of railway inspectors.

be due to our relative sanction mechanism. An absolute sanction mechanism holds the level of requested contribution constant whereas in our case when the contributions of others fall, the threshold of how much an individual should contribute drops sharply. This difference in the expected cost of sanction could also explain that we find a larger magnitude of differences across treatments compared to Kastlunger *et al*. (2009) who also manipulate the frequency of audits. Nevertheless, if contributions adjust to the level of beliefs, we should still observe a large between-treatment difference since beliefs on audits do not depend on the severity of fines.

Another interesting extension would be to test the optimal duration of sequences with and without audits. We could also study how contributions are affected by reintroducing a sequence with intermittent or continuous audits after a sequence with no audits. Finally, reversing our design (moving from no audit to continuous or irregular audits) would also help understand the residual effects of periods with no or rare audits after more severe audit policies are introduced. It is likely that the introduction of systematic audits (i.e. crackdowns) would lead to higher contributions compared to an intermittent audit regime. Testing this hypothesis would help to characterize more fully the conditions under which intermittent auditing schemes are optimal.

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TABLES AND FIGURES

Table 1. Summary of the experimental sessions

Session number	Treatment	Number of audits	Sanction coefficient	Belief elicitation	Number of participants
1	Continuous	22	1.25	No	18
2	Intermittent 7	7	3.75	No	15
3	Continuous	22	1.25	No	12
4	Intermittent 7	7	3.75	No	15
5	Continuous	22	1.25	Yes	15
6	Continuous	22	1.25	Yes	18
7	Intermittent 7	7	3.75	Yes	15
8	Intermittent 7	7	3.75	Yes	15
9	Intermittent 5	5	3.75	Yes	15
10	Intermittent 5	5	3.75	Yes	18
11	Intermittent 3	3	3.75	Yes	18
12	Intermittent 7 Low Fine	7	1.25	Yes	15
13	Intermittent 3	3	3.75	Yes	9
14	Intermittent 7 Low Fine	7	1.25	Yes	12
15	Continuous High Fine	22	3.75	Yes	21
16	Continuous High Fine	22	3.75	Yes	21
17	Continuous 19	19	1.25	Yes	18
18	Continuous 19	19	1.25	Yes	21

Table 2. Summary statistics on beliefs, contributions and payoffs

Variables	Periods	*Continuous treatments*			*Intermittent treatments*			
		Cont	*High Fine*	*Cont 19*	*Int 7*	*Int 5*	*Int 3*	*Int 7 Low Fine*
Beliefs (0-100)	1-22	**67.3 (32.6)**	70.9 (33.7)	56.1^b (31.2)	47.6^a (28.6)	48.4^a (26.4)	42.6^a (33.1)	49.7^a (29.7)
	23-50	**31.6 (34.4)**	27.6 (34.4)	41.4^c (34.5)	44.5^b (29.7)	41.3 (29.0)	30.3 (34.0)	41.3 (33.2)
	All	**47.3 (38.0)**	46.6 (40.3)	47.9 (33.9)	45.8 (29.3)	44.4 (28.1)	35.7^a (34.1)	45.0 (32.0)
Contributions (0-20)	1-22	**15.3 (5.3)**	18.1^a (3.6)	12.8 (6.9)	15.4 (5.7)	14.9 (5.4)	12.2^b (6.1)	11.7^b (6.6)
	23-50	**8.2 (7.4)**	12.8^c (8.2)	10.3 (8.7)	13.9^a (6.5)	12.5^b (6.7)	9.8 (6.3)	8.4 (6.9)
	All	**11.3 (7.4)**	15.1^c (7.1)	11.4 (8.1)	14.6^a (6.2)	13.5 (6.3)	10.8 (6.3)	9.9 (6.9)
Negative deviation	1-22	**-4.3 (3.8)**	-4.4 (3.5)	-4.8 (4.1)	-5.3 (4.9)	-4.9 (4.3)	-5.5 (4.9)	-6.2 (4.6)
from average contribut.	23-50	**-5.7 (4.6)**	-6.5 (4.7)	-6.3 (5.2)	-5.3 (4.9)	-5.0 (4.6)	-4.5 (3.6)	-5.8 (4.5)
$(c_i - \bar{c})\, if\, c_i < \bar{c}$	All	**-5.2 (4.4)**	-5.8 (4.4)	-5.6 (4.8)	-5.3 (4.9)	-5.0 (4.5)	-4.9 (4.2)	-6.0 (4.6)
Payoffs	1-22	**25.9 (3.8)**	26.0 (7.8)	24.5 (4.2)	25.1 (9.1)	25.6 (7.7)	25.0 (6.9)	24.7 (5.2)
	23-50	**24.1 (5.3)**	26.4^b (5.4)	25.1 (5.6)	26.9^a (4.6)	26.2^a (4.7)	24.9 (4.4)	24.2 (5.2)
	All	**24.9 (4.8)**	26.2 (6.6)	24.9 (5.1)	26.1^a (7.0)	26.0 (6.2)	24.9 (5.6)	24.5 (5.2)
Number of observations		3150	2100	1950	3000	1650	1350	1350

Note: Numbers indicate mean values. Standard deviations are in parentheses. The significance levels of two-tailed Mann–Whitney tests are represented by *a*, *b*, and *c*, *with* $p<0.01$, $p<0.05$, $p<0.10$, respectively. Continuous treatment (in bold) is the reference treatment.

Table 3. Determinants of beliefs (Tobit models with robust standard errors clustered at the group level)

Dependent variable:		*Periods*		*Continuous treatments*			*Intermittent treatments*			
Beliefs	*1-22*	*23-50*	*1-50*	*Cont*	*High Fine*	*Cont 19*	*Int 7*	*Int 5*	*Int 3*	*Low Fine*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Intermittent 7	-27.31***	22.30**	-0.30	-	-	-	-	-	-	-
	(6.87)	(9.35)	(4.37)							
Intermittent 5	-27.40***	17.14	-3.29	-	-	-	-	-	-	-
	(6.65)	(10.97)	(6.32)							
Intermittent 3	-34.60***	0.18	-16.13***	-	-	-	-	-	-	-
	(6.38)	(10.36)	(5.65)							
Intermittent Low Fine	-25.32***	17.85	-1.96	-	-	-	-	-	-	-
	(7.40)	(11.05)	(6.38)							
Continuous High Fine	8.50	-5.80	0.82	-	-	-	-	-	-	-
	(7.70)	(11.29)	(5.15)							
Continuous 19	-17.80***	13.59	-0.87	-	-	-	-	-	-	-
	(6.70)	(10.13)	(5.44)							
Anchor	-	-	-	1.56***	1.82***	1.00***	0.67***	0.88***	1.13***	0.66***
				(0.20)	(0.22)	(0.11)	(0.18)	(0.20)	(0.17)	(0.13)
Negative prediction	-	-	-	<0.01***	<0.01***	<0.01	<0.01	<0.01*	<0.01	-0.00
error (alpha)				(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(0.00)
Positive prediction	-	-	-	<0.01***	<0.01***	<0.01	<0.01	<0.01	<0.01	<0.01
error (beta)				(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)	(<0.01)
Loss index	1.75	0.82	1.29	1.50	-2.55	3.75**	-1.87	-1.42	2.83	-1.29
	(2.03)	(2.33)	(1.82)	(1.12)	(1.86)	(1.55)	(2.32)	(1.92)	(2.66)	(1.49)
Risk index	-0.92	-1.28	-1.13	-0.73	0.28	-0.84*	-0.79**	-0.62	1.02*	-0.37
	(0.89)	(0.97)	(0.75)	(0.49)	(0.75)	(0.49)	(0.36)	(0.60)	(0.58)	(1.60)

Table 3. (*continued*)

	Periods			*Continuous treatments*			*Intermittent treatments*			
	1-22	*23-50*	*1-50*	*Cont*	*High Fine*	*Cont 19*	*Int 7*	*Int 5*	*Int 3*	*Low Fine*
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Ambiguity aversion	0.70	0.60	0.66	0.10	-0.31	-0.35	1.00**	1.02*	0.96	-0.46
	(0.70)	(0.72)	(0.53)	(0.53)	(0.46)	(0.59)	(0.41)	(0.57)	(1.10)	(0.62)
Male	-5.41	-0.91	-3.10	-0.55	-3.82	-2.45	-2.33	2.24	-11.25**	-4.19
	(3.76)	(4.07)	(2.91)	(3.29)	(2.64)	(2.46)	(3.85)	(4.00)	(4.52)	(2.69)
Age	0.44	1.34***	0.94**	2.14*	0.35	-0.13	0.49***	1.31***	0.77*	-0.07
	(0.46)	(0.49)	(0.46)	(1.18)	(0.28)	(0.24)	(0.19)	(0.49)	(0.43)	(0.29)
Period	1.14***	-1.19***	-0.97***	0.36***	0.52**	-0.17*	-0.14	-0.12*	-0.26***	-0.29***
	(0.25)	(0.16)	(0.14)	(0.10)	(0.25)	(0.09)	(0.11)	(0.07)	(0.10)	(0.07)
Constant	57.18***	43.00**	56.32***	-85.69***	-51.98**	2.04	24.31***	-5.58	-30.82*	32.62**
	(15.85)	(19.60)	(15.72)	(29.43)	(20.32)	(8.91)	(9.04)	(12.10)	(17.04)	(15.51)
Observations	5082	6468	11550	1617	2058	1911	1470	1617	1323	1323
Left/right censored obs.	543/1081	1936/589	2479/1670	433/344	616/556	401/209	198/113	257/104	349/120	197/133
Log-pseudo-likelihood	-19517.91	-23285.52	-43414.61	-4729.15	-5035.17	-6715.30	-5924.01	-6277.45	-4599.19	-5141.38
Pseudo R^2	0.02	0.01	0.01	0.14	0.20	0.08	0.04	0.06	0.08	0.06

Note: In models (1) to (3), the Continuous treatment is the omitted reference category. Robust standard errors are in parentheses, clustered by group. The data include only the sessions with belief elicitation. *** indicate p<0.01, ** p<0.05, * p<0.1.

Table 4. Determinants of contributions (Tobit models with robust standard errors clustered at the group level)

Dependent variable:	*Periods 1-22*		*Periods 23-50*		*Periods 1-50*	
Contribution	(1)	(2)	(3)	(4)	(5)	(6)
Intermittent 7	-0.40 (0.91)	-0.15 (0.95)	2.66***(0.75)	2.31***(0.82)	1.27*(0.70)	1.31*(0.79)
Intermittent 5	-0.37 (1.08)	-0.97 (1.04)	2.78***(0.96)	1.40 (0.94)	1.32 (0.87)	0.35 (0.88)
Intermittent 3	-1.64*(0.84)	-2.08***(0.78)	1.47**(0.69)	0.74 (0.66)	0.10 (0.59)	-0.48 (0.55)
Intermittent Low Fine	-2.29***(0.86)	-2.89***(0.78)	0.98 (0.82)	-0.40 (0.71)	-0.47 (0.63)	-1.48***(0.56)
Continuous High Fine	3.64**(1.42)	2.50*(1.41)	3.98***(1.32)	3.31***(1.28)	3.66***(1.15)	2.68**(1.16)
Continuous 19	-0.47 (1.11)	-1.27 (1.11)	1.86 (1.40)	0.49 (1.40)	0.82 (1.09)	-0.31 (1.11)
Belief elicitation	-0.60 (0.94)	-	-0.21 (0.77)	-	-0.34 (0.72)	-
Belief	-	0.02***(0.01)	-	0.05***(0.01)	-	0.04***(0.01)
Others' mean contribution in *t*-1	1.00***(0.08)	1.00***(0.08)	1.12***(0.08)	1.12***(0.08)	1.10***(0.07)	1.09***(0.07)
Loss index	0.29 (0.27)	0.25 (0.27)	0.32 (0.30)	0.29 (0.30)	0.30 (0.25)	0.26 (0.25)
Risk index	-0.01 (0.12)	-0.01 (0.12)	-0.12 (0.11)	-0.10 (0.11)	-0.06 (0.10)	-0.06 (0.10)
Ambiguity aversion	-0.01 (0.11)	-0.01 (0.11)	0.23**(0.11)	0.23**(0.11)	0.12 (0.09)	0.12 (0.09)
Male	0.15 (0.59)	0.19 (0.59)	-0.06 (0.64)	-0.02 (0.66)	0.05 (0.51)	0.10 (0.54)
Age	-0.11***(0.04)	-0.11***(0.04)	-0.05 (0.05)	-0.07 (0.05)	-0.08**(0.04)	-0.09**(0.04)
Period	0.02 (0.02)	0.01 (0.02)	-0.10***(0.02)	-0.08***(0.02)	-0.07***(0.01)	-0.05***(0.01)
Constant	4.05*(2.33)	3.08 (2.13)	2.19 (2.63)	0.73 (2.55)	2.39 (2.13)	1.10 (1.99)
Observations	6111	6111	8148	8148	14259	14259
Left/right-censored obs.	363/2281	363/2281	1567/2084	1567/2084	1930/4365	1930/4365
Log-pseudo-likelihood	-13590.70	-13563.30	-19160.40	-19054.81	-32914.01	-32760.22
Pseudo R^2	0.09	0.10	0.10	0.11	0.11	0.12

Note: The Continuous treatment is the omitted reference category. Robust standard errors are in parentheses, clustered by group. All sessions included. *** indicate p<0.01, ** p<0.05, * p<0.10.

Table 5. Determinants of the evolution of contribution between *t*-1 and *t* (OLS models with robust standard errors clustered at the group level)

Dependent variable: *Evolution of contribution between t-1 and t*	*Periods 1-22* (1)	*Periods 23-50* (2)	*Periods 1-50* (3)
Evolution of beliefs between *t*-1 and *t*	0.03** (0.01)	0.06***(0.02)	0.05***(0.01)
Evolution of belief * Intermittent 7	0.04***(0.02)	<0.01 (0.02)	0.02 (0.02)
Evolution of belief * Intermittent 5	0.04 (0.02)	<0.01 (0.03)	0.02 (0.02)
Evolution of belief * Intermittent 3	0.05* (0.03)	0.03 (0.02)	0.03 (0.02)
Evolution of belief * Intermittent Low Fine	0.03**(0.01)	0.01 (0.03)	0.02 (0.02)
Evolution of belief * Continuous High Fine	-0.02 (0.01)	-0.01 (0.02)	-0.02 (0.02)
Evolution of belief * Continuous 19	-0.02 (0.02)	-0.03 (0.03)	-0.02 (0.02)
Punished in *t*-1	2.92***(0.27)	-	-
Punished in *t*-1* Intermittent 7	0.53 (0.95)	-	-
Punished in *t*-1* Intermittent 5	2.13***(0.75)	-	-
Punished in *t*-1* Intermittent 3	-0.03 (0.63)	-	-
Punished in *t*-1* Intermittent Low Fine	0.04 (0.54)	-	-
Punished in *t*-1* Continuous High Fine	0.38 (0.60)	-	-
Punished in *t*-1* Continuous 19	-0.15 (0.50)	-	-
Others' mean contribution in *t*-1	0.04***(0.01)	0.02***(0.01)	0.03***(0.01)
Loss index	<-0.01 (0.03)	<0.01 (0.02)	-0.04 ***(0.01)
Risk index	<0.01 (0.01)	<0.01 (0.01)	<0.01 (0.01)
Ambiguity aversion	<0.01 (0.01)	<0.01* (0.01)	<0.01 (0.01)
Male	0.08 (0.07)	-0.05 (0.04)	-0.02 (0.02)
Age	<0.01 (0.01)	<0.01 (0.01)	<0.01 (0.01)
Period	<0.01* (0.01)	<0.01 (0.01)	<0.01*(0.01)
Constant	-0.86**(0.34)	-0.22 (0.27)	-0.21**(0.13)
Observations	4851	6468	11319
R^2	0.18	0.10	0.10

Note: Robust standard errors are in parentheses, clustered by group. All sessions included. *** indicate p<0.01, ** p<0.05, * p<0.10.

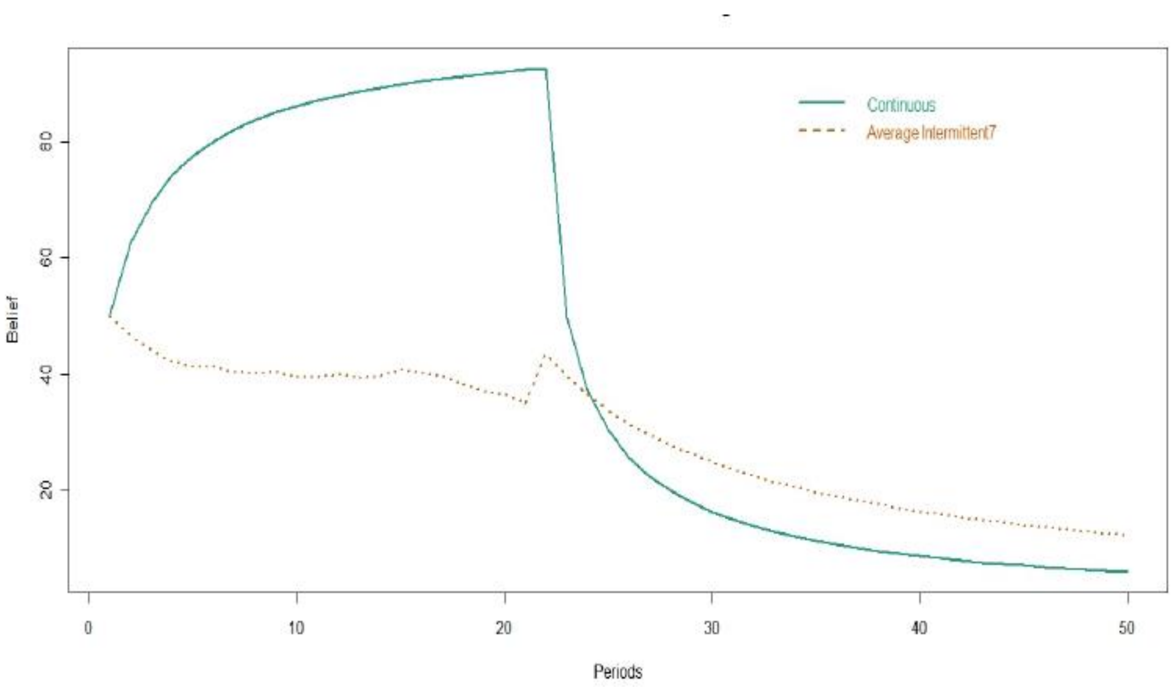
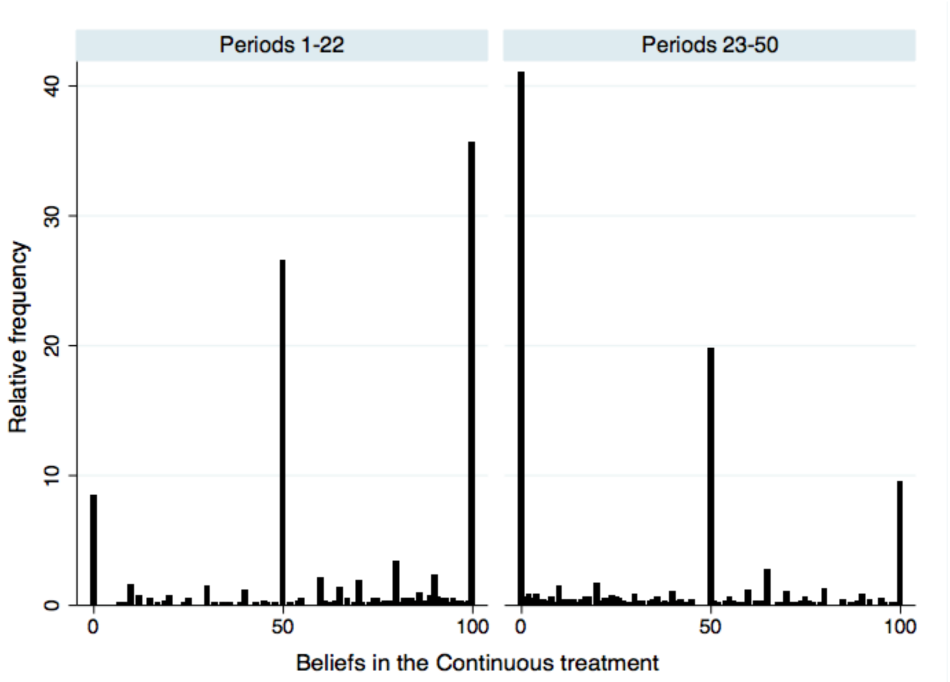


Fig. 1. Simulation of belief learning in the Continuous and Intermittent 7 treatments (with α=β=0.5 in the Continuous treatment and α=β=0.2 in the Intermittent 7 treatment)



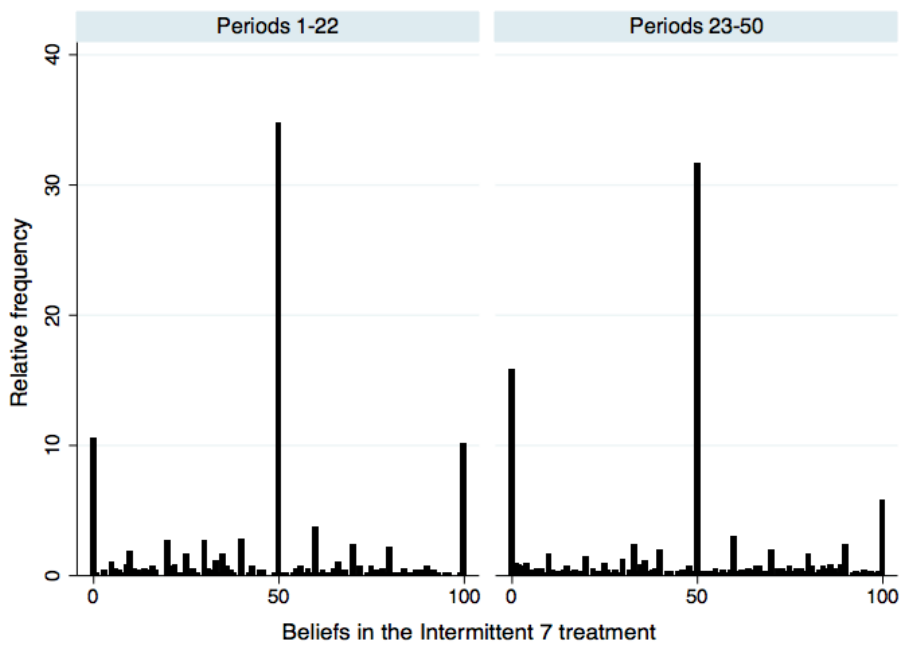
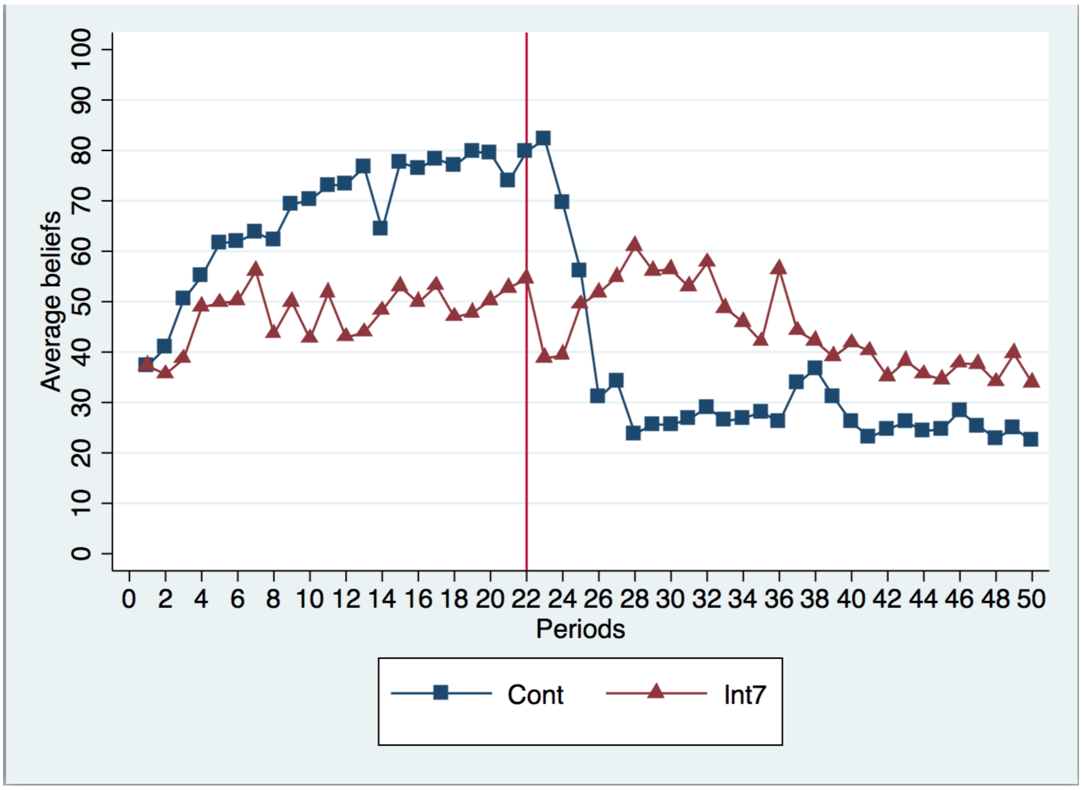
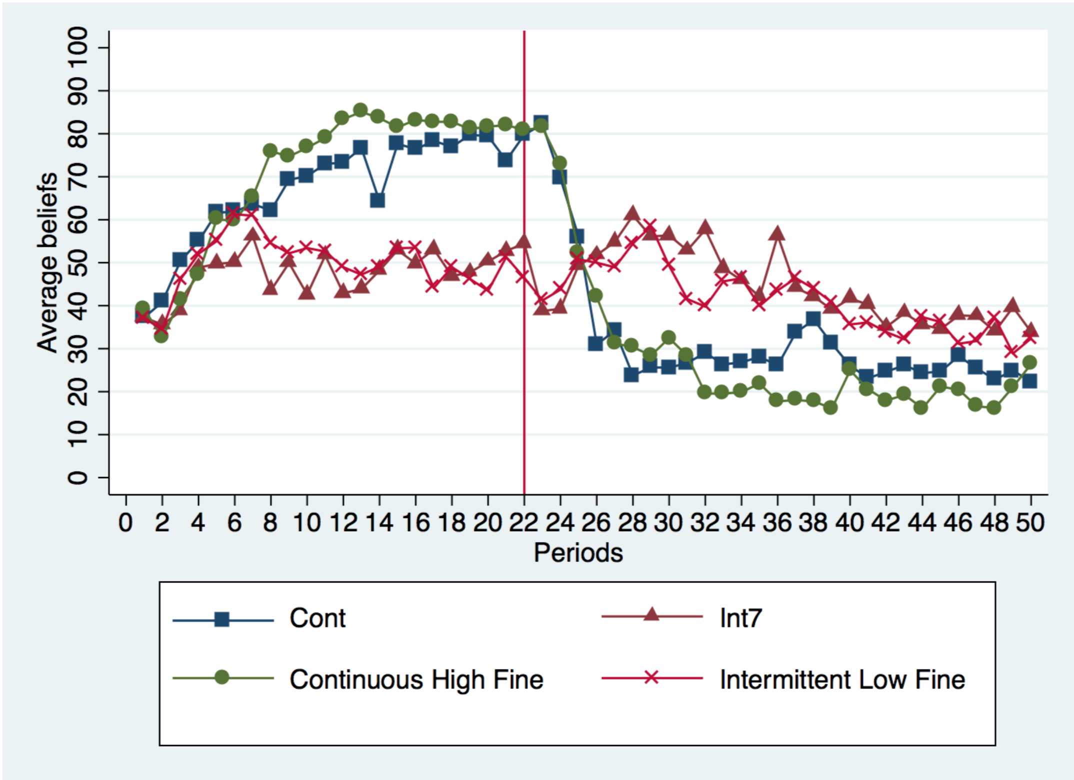


Fig. 2. Distribution of beliefs in the Continuous and Intermittent 7 treatments

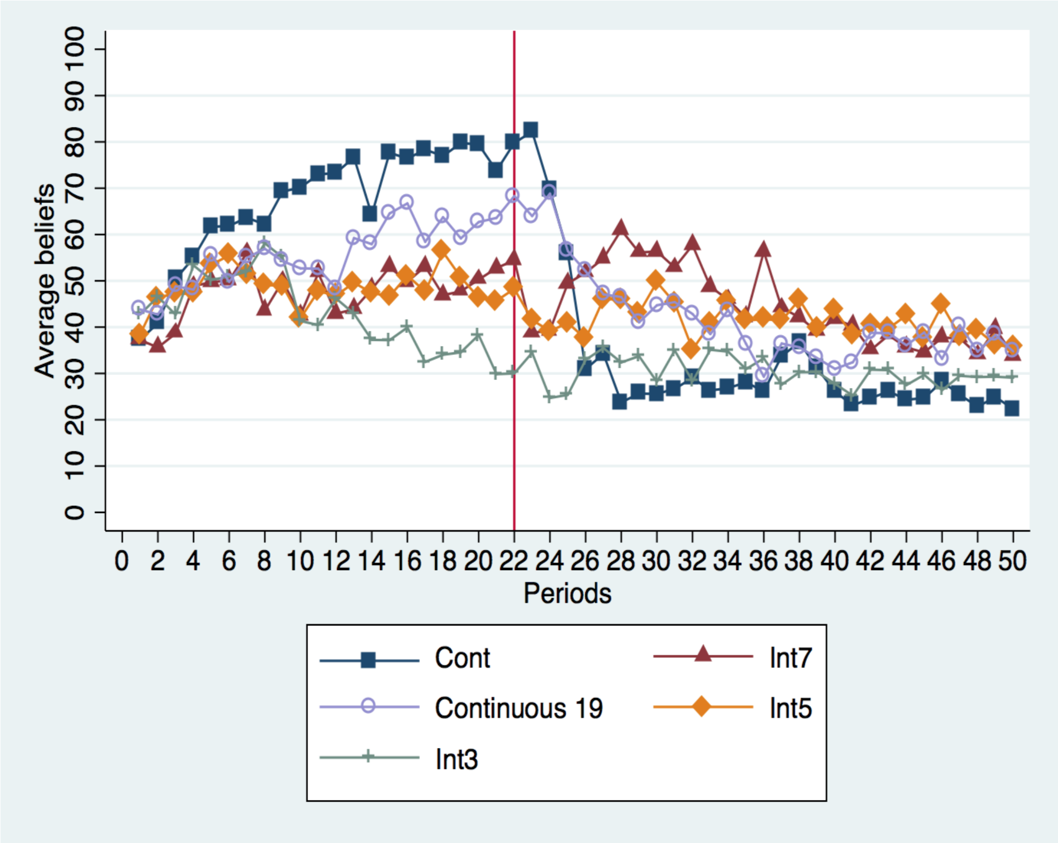


a) Mean beliefs in the Continuous and Intermittent 7 treatments



b) Mean beliefs under various sanction coefficients

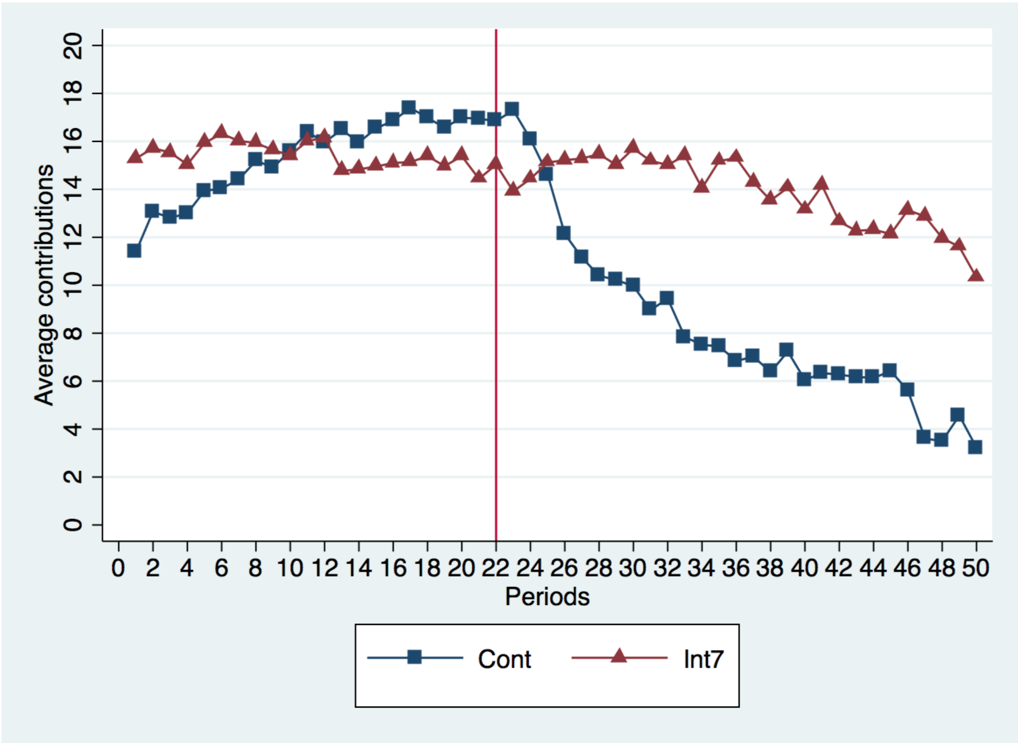
Fig. 3. Evolution of mean beliefs over time



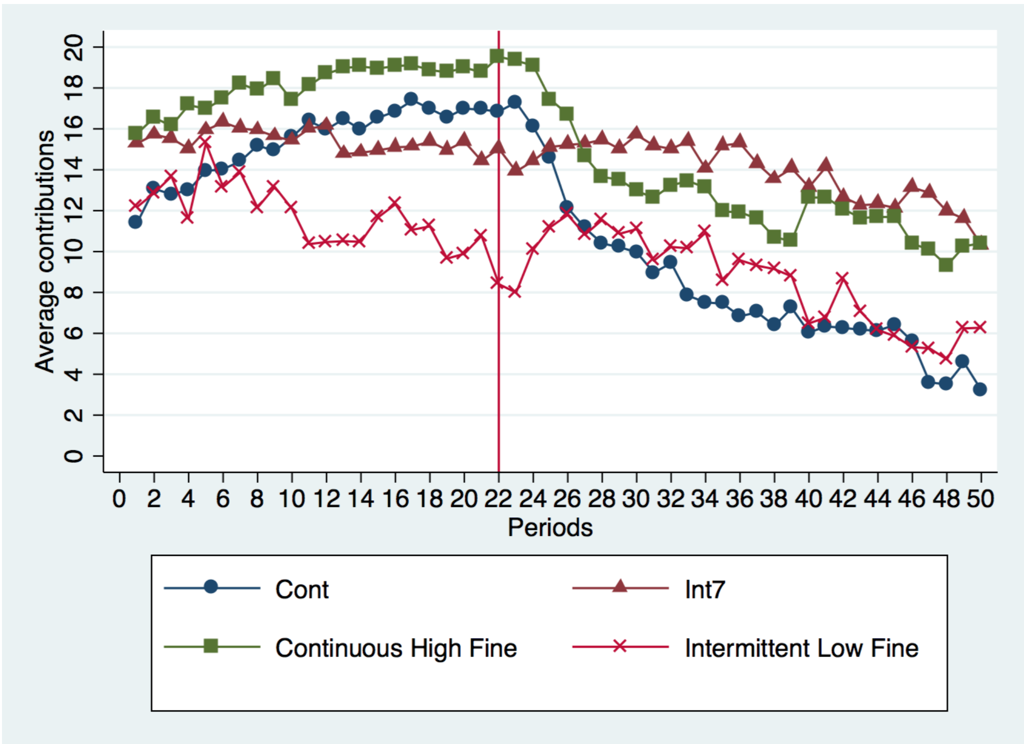
c) Mean beliefs under various audit frequencies

Fig. 3. Evolution of mean beliefs over time (continued)

Note: The vertical line in each graph indicates the period from which audits are definitely withdrawn

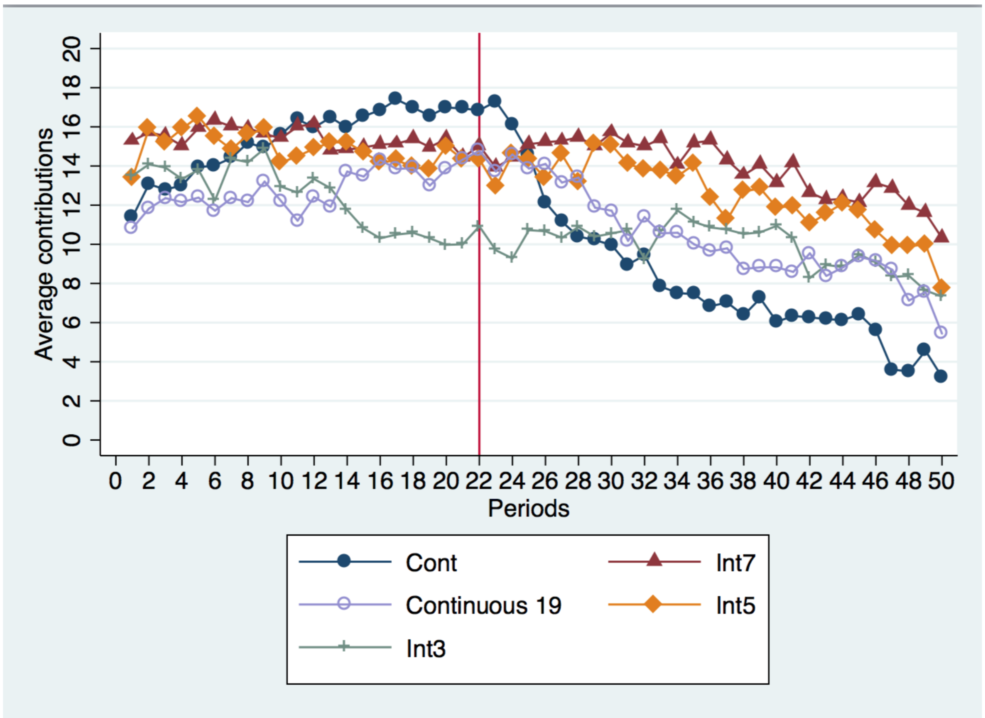


a) Mean contributions in the Continuous and Intermittent 7 treatments



b) Mean contributions under various sanction coefficients

Fig. 4. Evolution of mean contributions over time



c) Mean contributions under various audit frequencies

Fig. 4. Evolution of mean contributions over time (continued)

Note: The vertical line in each graph indicates the period from which audits are definitely withdrawn.

APPENDIX A. Proof of the equilibrium play

Consider a one-shot game. Suppose that the mean contribution of the other group members is $\bar{c}_{-i}$.

Player *i*'s contribution can be presented either as:

$\bar{c}_{-i} + \alpha_i$ if player *i* over-contributes by α_i, $a_i > 0$, (1)

or as:

$\bar{c}_{-i}$-β_i if player *i* under-contributes by β_i, $\beta_i \geq 0$. (2)

If player *i* chooses to over-contribute, his payoff function can be written:

$$p(\bar{c}_{-i} + a_i) = 20 + 0.5\,\bar{c}_{-i} - 0.5a_i \tag{3}$$

This reaches a maximum when $\alpha_i = 0$. Thus, player *i* has no incentive to over-contribute.

If instead, player *i* chooses to contribute less than his group members and if there is an audit, player *i* will be sanctioned. Suppose an audit occurs with probability *p*. δ is the fine coefficient. Then, player *i*'s payoff function becomes:

$$p(\bar{c}_{-i} - b_i) = 20 + 0.5\bar{c}_{-i} + (0.5 - \mathrm{p}^*d)\, b_i \tag{4}$$

If *p* is known, it pays to contribute less than the mean of others only when $p^*\delta < 0.5$.

Thus, given the others' mean contribution, when $p^*\delta < 0.5$ player *i*'s best response is to free-ride completely (β_i =20, c_i =0). Otherwise, if $p^*\delta \geq 0.5$, player *i* should contribute exactly the same amount as the average of others ($c_i = \bar{c}_{-i}$).

There are 20 Nash equilibria corresponding to each possible mean contribution. If we consider the concept of payoff dominance, it is clear that partial contribution will never maximize profits in this linear public goods game. Thus, full contribution is a payoff-dominant Nash equilibrium.

Since δ is common knowledge but *p* is unknown, the player has to form beliefs about the value of $\mathrm{p}^*\delta$ that only depend on beliefs of the value of *p*. Let us assume that players have the same prior beliefs on the probability of being audited and expect that others have the same priors, and let us assume risk neutrality. In the Continuous, Continuous 19 and the Intermittent 7 Low Fine treatment, players should all contribute 20 if they believe that *p* is larger than 0.4, and 0 otherwise. In the Continuous High Fine and Intermittent 7/5/3 treatments, players should all contribute 20 if they believe that *p* is larger is 0.13, and 0 otherwise. Using backward induction, the single shot outcome should be also observed in the repeated game with 50 periods because the game is finitely repeated.

Let us now consider risk preferences. Payoffs should be replaced with utility. Risk/ambiguity/loss aversion will push contribution upward to reduce the risk – and the disutility- of being sanctioned. For example, if player *i* is risk neutral and he believes that others contribute 0, he will not contribute more than 0. This is not the case if he is risk/ambiguity/loss averse: in order to avoid potential sanctions, he will choose to contribute a bit more than 0. We further assume that players are homogeneous, then others will think the same way, and thus this will drive all players to contribute upwards.■

APPENDIX B. Instructions for the Continuous treatment *(Original instructions in French)*

You are taking part in an experiment in economics during which you can earn money. Your earnings will depend on your decisions and on the decisions of the other participants with whom you will interact. It is therefore important to read these instructions with attention. You will also be given 4 Euros for showing-up on time.

This session consists of several independent parts. We have distributed the instructions for the first part; you will receive later the instructions for the next parts.

At the end of the session, your earnings from the various parts will be added. You will be paid individually and in cash in a separate room, by somebody who is not aware of the content of the experiment.

Throughout the session, it is strictly forbidden to communicate with the other participants.

Part 1

Your computer screen will present you successively with two urns that contain each ten balls, either yellow or blue.

- The first urn contains 5 blue balls and 5 yellow balls.
- The second urn contains also blue and yellow balls but in unknown proportions.

For each urn, you must make 20 successive choices between drawing a ball from the urn with replacement or earning a certain amount of money. **If you draw a yellow ball from the urn, you earn € 5; if you draw a blue ball from the urn, your earn € 0.**

We propose you **20 certain possible amounts, from € 0.25 to € 5**, as shown in the Table below. For each urn, you must make a decision for each of the 20 proposals. Only one of these decisions will matter for determining your earnings in this part, as explained below.

Once you have completed each of the two tables, please validate your choice by pressing the "OK" button.

1	O I choose the certain amount of € 0.25	O I choose to draw a ball
2	O I choose the certain amount of € 0.50	O I choose to draw a ball
3	O I choose the certain amount of € 0.75	O I choose to draw a ball
4	O I choose the certain amount of € 1	O I choose to draw a ball
5	O I choose the certain amount of € 1.25	O I choose to draw a ball
6	O I choose the certain amount of € 1.50	O I choose to draw a ball
7	O I choose the certain amount of € 1.75	O I choose to draw a ball
8	O I choose the certain amount of € 2	O I choose to draw a ball
9	O I choose the certain amount of € 2.25	O I choose to draw a ball
10	O I choose the certain amount of € 2.50	O I choose to draw a ball
11	O I choose the certain amount of € 2.75	O I choose to draw a ball
12	O I choose the certain amount of € 3	O I choose to draw a ball
13	O I choose the certain amount of € 3.25	O I choose to draw a ball
14	O I choose the certain amount of € 3.50	O I choose to draw a ball
15	O I choose the certain amount of € 3.75	O I choose to draw a ball
16	O I choose the certain amount of € 4	O I choose to draw a ball
17	O I choose the certain amount of € 4.25	O I choose to draw a ball
18	O I choose the certain amount of € 4.50	O I choose to draw a ball
19	O I choose the certain amount of € 4.75	O I choose to draw a ball
20	O I choose the certain amount of € 5	O I choose to draw a ball

How do we determine your earnings in this part?

At the end of the session, the computer program will randomly determine which urn is used for payment. Next, for this urn, it will randomly draw a number between 1 and 20 to determine which of your 20 decisions will matter for determining your earnings.

- For this decision, if you have chosen the certain amount, this amount will be added up to your other earnings from the experiment.
- If you have chosen to draw a ball, the computer program will draw the ball from the selected urn. If a yellow ball is drawn, €5 will be added to your other earnings from the experiment.

If you have any question regarding these instructions, please raise your hand and we will answer your questions in private.

Instructions for Part 2 *(distributed after completion of Part 1)*

In this part, you must make six successive choices on your computer between accepting and refusing to take part in a lottery with two possible outcomes: yellow or blue.

o If you refuse the draw, you win €0 and you lose €0.

o If you accept the draw, you win €6 if the computer program randomly draws the yellow color (which comes with a one in two chance) and you lose a certain amount if the computer program draws the blue color (which happens with a one in two chance).

In each successive decision, the amount of the gain is always €6 and the amount of the loss ranges from €2 to €7.

Your computer screen will display the following Table and you will make a decision on each line.

	I accept	I reject
1. If Yellow is drawn, I earn €6. If Blue is drawn, I lose €2.	O	O
2. If Yellow is drawn, I earn €6. If Blue is drawn, I lose €3.	O	O
3. If Yellow is drawn, I earn €6. If Blue is drawn, I lose €4.	O	O
4. If Yellow is drawn, I earn €6. If Blue is drawn, I lose €5.	O	O
5. If Yellow is drawn, I earn €6. If Blue is drawn, I lose €6.	O	O
6. If Yellow is drawn, I earn €6. If Blue is drawn, I lose €7.	O	O

Please look at the first line. In this line, you choose between accepting and rejecting a draw which gives 1/2 chance to win €6 and 1/2 chance to lose €2. In the next line, you choose between accepting and rejecting a draw which gives 1/2 chance to win €6 and the same chance to lose €3. And so on until the 6th decision.

How we determine your earnings in this part?

At the end of the session, the computer program will randomly choose one of your six decisions.

o If you have refused to draw for this decision, your gain for this part is €0.

o If you have accepted to draw for this decision, the program randomly draws one of two colors. If the yellow color is drawn, your gain for this part is €6 that will be added to your other earnings in the session. If the blue color is drawn, you will lose a certain amount that will be deducted from your other earnings in the session.

If you have any questions about this instruction, raise your hand and we will answer these questions in private.

Instructions for Part 3 *(distributed after completion of Part 2)*

This part consists of 50 periods.

All the transactions during this part are conducted in ECU (Experimental Currency Units), according to the following rules:

- o Your total payoff in ECU for this part consists of the sum of your payoffs in each of the 50 periods comprising this part.
- o ECU will be converted into Euros at the rate: 150 ECU = 1 Euro.

At the end of the session, the total amount of ECU you have earned during this part will be converted to Euros and added to your other earnings.

At the beginning of this part, the participants are divided into groups of three. You will therefore interact with two other participants. **During the 50 periods, you will interact with the same persons**. You will never be informed of the identity of these persons.

Description of each period

At the beginning of each period, each group member receives an endowment of 20 ECU.

The three participants belonging to a group can participate in a project by investing in a public account that will be shared equally among them. The amount of this public account is determined by the sum of the individual investments of the three members of the group.

In some periods, the group members' investments can be reviewed by the computer program. In case of a review, if a group member has invested less than the average of the two other group members, his payoff is reduced.

The details of each period are described below.

1) You receive an endowment of 20 ECU. You, as well as the two other group members, simultaneously decide how much of your endowment you will invest in the public account, by indicating a number between 0 and 20. The amount of your endowment that is not invested in the public account is assigned to your private account. To validate your choice, you must press the OK button.

After all group members have made their decision, your screen will display the total amount of ECU invested in the project by the group members (including your own investment).

Your payoff consists of two parts:

- the amount of your endowment that you have kept for yourself in your private account (i.e. 20 – your investment),
- the income from the project: this income represents 50% of the total investment of all three group members in the public account.

Your payoff in ECU is computed as follows:

(20 - your investment) + 50%* (total investment of the group)

The payoff of each group member is calculated in the same way, which means that each group member receives the same income from the project.

Suppose the total investment of all group members is 50 ECU. In this example each member of the group receives an income from the project of 50% of 50 ECU = 25 ECU. If the total investment is 10 ECU, then each member of the group receives an income of 50% of 10 ECU = 5 ECU from the project.

For each ECU of your endowment that you keep on your private account you earn an income of 1 ECU. Every ECU you invest in the public account instead increases the total investment by one ECU. The income from the project will increase by 0.5 ECU per person and so, the total income of the group from the project rises by 1.5 ECU. This means that your investment in the public account also increases the income of the other group members.

On the other hand you will earn money from each ECU invested by the other members in the project. For each ECU invested by any group member you earn 50% (1) = 0.5 ECU.

2) In some periods, the investments of the group members are reviewed by the computer. **You do not know in advance which periods are reviewed and how many periods are reviewed.**

- If there is no review, the period ends and your payoff of the period is not modified.
- In case of a review, there are two possible situations.

- If you have invested in the public account less than the average investment of the two other group members, your payoff is reduced. The payoff reduction amounts to 1.25 times the difference between the mean investment of the two other group members and your investment.

- If you have invested the same amount or more than the average investment of two other group members, your payoff is not modified.

To recapitulate, in each period, your payoff in ECU is calculated as follows:

- (20 - your investment) + 50%* (total investment of the group) – 1.25 * (mean investment of the two other group members – your investment) in case of a review and if your investment is lower than the average investment of the two other members

- (20 - your investment) + 50%* (total investment of the group), otherwise

In periods with a review, your payoff can be negative. In case your payoff would be negative at the end of the 50 periods, this loss would be deducted from your other earnings.

3) At the beginning of each period, before you choose your investment, we ask you to estimate the number of chances (from 0 to 100) that the investments will be reviewed at the end of this period. For example, if you believe that there are X chances out of 100 that they will be reviewed, then enter the number X.

At the end of the session, the computer program will randomly draw one of the 50 periods and it will compare your prediction to the existence or not of a review in this period. You will receive an additional payoff that will depend on the precision of your estimate. We will pay you for your prediction as follows:

Suppose you predict that there are 20 chances out of 100 that there will be a review and therefore 80 chances out of 100 that there will be no review. Suppose now that there was actually no review in this period. In that case, your payoff will be

$$\text{Prediction payoff} = \left[2\mathit{Euro} - \left(1 - \frac{80}{100}\right)^2 - \left(\frac{20}{100}\right)^2 \right]$$

In other words, we will give you a fixed amount of 2 Euros from which we will subtract an amount that depends on how inaccurate your prediction was. To do this, when we find out whether a review occurred or not, we will take the number you assigned to the situation that actually occurred, in this case 80% for no review, subtract if from 100% and square it. We will then take the number you assigned to the situation that did not occur, in this case the 20% that you assigned to review, and square it also. These two squared numbers will then be subtracted from the 2 Euros we initially gave you to determine your final prediction payoff.

Note that the worst you can do under this payoff scheme is to state that you believe that there is a 100% chance that a certain situation will occur and assign 100% to that situation when in fact the other situation actually occurs. Here your payoff from prediction will be 0. Similarly, the best you can do is to guess correctly and assign 100% to that situation which turns out to be the actual situation. Here your payoff will be 2 Euros.

<u>However, since your prediction is made before you know whether there is a review or not in the period, the best thing you can do to maximize the expected size of your prediction payoff is to simply state your true belief about the number of chances there will be a review or not. Any other prediction will decrease the amount you can expect to earn as a prediction payoff.</u>

We will pay your prediction in one of the 50 periods. As you do not know in advance which period will be randomly selected at the end of the session for payment of prediction payoff, please pay the same attention to each of your 50 predictions.

4) Information

After all group members have made their prediction and their investment decision, you are informed about the total amount invested in the public account.

You also learn whether the investments were reviewed or not, whether your payoff has been reduced and by how much, and your final payoff for the period.

You are not informed whether other group members' payoffs have been reduced and their final payoffs.

* * *

At the end of a period, the next period starts automatically. You receive a new endowment of 20 ECU, you report your prediction about the existence of a review in this period, and you decide on your investment in a public account.

After completing the 50 periods, you will be asked to answer a final brief questionnaire. Then, you will be invited to leave the room and to proceed to the payment room.

Please read these instructions again and answer the questionnaire that has been distributed; we will check your answers individually. If you have any questions about these instructions, please raise your hand. We will answer your questions in private.